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PREFACE.

In accordance with the rules of the National Academy of Sciences, it is the duty of the President, upon the death of any member, to select some one to prepare a biographical memoir of the deceased. Such memoirs have been collected in permanent form from time to time, for the use of the Academy and for distribution among the large libraries of the country.

The present is the fifth volume, and contains fourteen memorial sketches, each supplemented by a list of all the more important scientific publications of the deceased. In this volume each biography is accompanied by a likeness and *fac simile* of signature of the deceased member, adding greatly to its permanent interest and value. It should be stated that several of the memoirs here brought together for the first time have been published elsewhere by scientific institutions or learned societies.

With the close of this volume seventy-seven of these biographies will have been published by the Academy.

HOME SECRETARY.

WASHINGTON, D. C., JANUARY 10, 1905.

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Prof. Joseph Henry.

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MEMOIR OF

JOSEPH HENRY

SIMON NEWCOMB

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MEMOIR

OF

JOSEPH HENRY.

BY

SIMON NEWCOMB.

READ BEFORE THE NATIONAL ACADEMY OF SCIENCES,

APRIL 21, 1880.

BIOGRAPHICAL MEMOIR OF JOSEPH HENRY.

In presenting to the Academy the following notice of its late lamented President the writer feels that an apology is due for the imperfect manner in which he has been obliged to perform the duty assigned him. The very richness of the material has been a source of embarrassment. Few have any conception of the breadth of the field occupied by Professor Henry's researches, or of the number of scientific enterprises of which he was either the originator or the effective supporter. What, under the circumstances, could be said within a brief space to show what the world owes to him has already been so well said by others that it would be impracticable to make a really new presentation without writing a volume. The Philosophical Society of this city has issued two notices which together cover almost the whole ground that the writer feels competent to occupy. The one is a personal biography—the affectionate and eloquent tribute of an old and attached friend; the other an exhaustive analysis of his scientific labors by an honored member of the society well known for his philosophic acumen.* The Regents of the Smithsonian Institution made known their indebtedness to his administration in the memorial services held in his honor in the Halls of Congress.

Under these circumstances the only practicable course has seemed to be to give a condensed *résumé* of Professor Henry's life and works, by which any small occasional gaps in previous notices might be filled. That in doing this the writer may repeat much that has already been better said by others is a fault which he hopes the Academy will pardon in view of the difficulty of avoiding it.

The interest which, in the light of modern theories of heredity,

* The scientific work of Joseph Henry: Bulletin of the Philosophical Society of Washington, vol. II, p. 230; Smithsonian Miscellaneous Collections, vol. xxi, pp. 205–425; A Memorial of Joseph Henry, published by order of Congress, 1880.

attaches to the ancestry of men possessing uncommon intellectual powers would naturally lead us to desire a knowledge of Professor Henry's ancestors. We have, however, no sufficient historical data for gratifying any desire of this kind. Little more can be said than that his grand-parents were of Scottish origin, and landed in this country about the beginning of the revolutionary war. Of his father little is known, and that little does not enable us to explain why he had such a son. His mother was a woman of great refinement, intelligence, and strength of character, but of a delicate physical constitution. Like the mothers of many other great men, she was of deeply devotional character. She was a presbyterian of the old-fashioned Scottish stamp, and exacted from her children the strictest performance of religious duties.

The son Joseph was born in Albany, on the 17th of December, either 1797 or 1799.* The doubt respecting the year has not yet been decisively settled. At the age of seven years he left his paternal home and went to live with his grandmother at Galway, where he attended the district school for three years. At the age of ten he was placed in a store kept by a Mr. Broderick, and spent part of the day in business duties and part at school. This position he kept until the age of fifteen. During these early years his intellectual qualities were fully displayed, but in a direction totally different from that which they ultimately took. He was slender in person, not vigorous in health, with almost the delicate complexion and features of a girl. His favorite reading was books of romance. The lounging-place for the young villagers of an evening was around the stove in Mr. Broderick's store. Here young Henry, although the slenderest of the group, was the central figure, retailing to those around him the stories which he had read, or which his imagination suggested. He was of a highly imaginative turn of mind, and seemed to live in the ideal world of the fairies.

At the age of fifteen he returned to Albany, and, urged by his imaginative taste, joined a private dramatic company, of which

* This uncertainty appears to have resulted from the difficulty of deciphering the faded record of date in the old family bible. The writer's personal examination of the extant material leads him to favor the earlier date, which he believes to have been the one to which Henry himself was inclined.

he soon became the leading spirit. There was every prospect of his devoting himself to the stage when, at the age of sixteen, accident turned his mental activities into an entirely different direction. Being detained indoors by a slight indisposition, a friend loaned him a copy of Dr. Gregory's lectures on Experimental Philosophy, Astronomy, and Chemistry. He became intensely interested in the field of thought which this work opened to him. Here in the domain of nature were subjects of investigation far more worthy of attention than anything in the ideal world in which his imagination had hitherto roamed. He determined to make the knowledge of this newly opened domain the great object of his life, but did not confine himself to any narrow sphere. He devoted himself immediately, with great ardor, to study. During the three years following he was successively English teacher, pupil of various masters, and a student at the Albany Academy. At about eighteen years of age he was recommended by Dr. Beck to the position of private tutor in the family of the patroon. He found this situation to be a very pleasant one, and was treated with great consideration by the family of Mr. Van Rensselaer. His duties required only his morning hours, so that he could devote his entire afternoons to mathematical and physical studies. In the former he went so far as to read the *Mécanique Analytique* of La Grange.

His delicate constitution now suffered so much from confinement and study that he accepted an invitation to go on a surveying expedition to the western part of the State. In this work his constitution was completely restored, and he returned home with a health and vigor which never failed him during the remainder of his long and arduous life. Soon after his return he was elected a professor at the Albany Academy. Here a new field was opened to him. It is one of the most curious features in the intellectual history of our country that, after producing such a man as Franklin, it found no successor to him in the field of science for half a century after his scientific work was done. There had been without doubt plenty of professors of eminent attainments who amused themselves and instructed their pupils and the public by physical experiments. But in the department of electricity, that in which Franklin took so prominent a position, it may be doubted whether they enunciated a single generalization which will enter into the history of the science. This interregnum closes with the researches now commenced by Professor Henry. His first published paper on the subject was read in 1827 before the Albany Institute, and is entitled, "On some modifications of the electro-magnetic apparatus." It consisted simply of a brief discussion of several forms of apparatus designed to exhibit the mutual action of the galvanic current and the magnet, but does not appear to comprise any discussions of new ideas. Two years later he published a topographical sketch of the State of New York, which also appeared in the Transactions of the Albany Institute. It comprises a brief sketch of the physical geography of the State with especial reference to the newly inaugurated canal system.

In 1831 he published in Silliman's Journal a paper on the development of great magnetic power in soft iron with a small galvanic element. This paper is in some sort a continuation of his first paper, the fundamental object of both being to show how the greatest development of magnetism could be obtained with the smallest battery. The ideas were suggested by the study of Schweigger's Galvanometer. He shows that in a piece of soft iron the magnetic power produced by the galvanic current may be greatly increased by increasing the number of coils. A still further improvement is made when, instead of passing a single coil between the two poles of the battery, a number of separate insulated wires are wound around the magnet, so that each shall form an independent connection. He was thus enabled with a battery of a single pair of small plates (4 by 6 inches) to form an electro-magnet which would lift a weight of 39 pounds. He also intimates that by winding a separate wire on each inch of the magnet a yet greater effect could be attained. This paper also contains the germ of the theory of electro-magnetic force, and of electrical resistance and quantity, though not developed in any generalized form. He explains that with one very long wire a combination of several plates must be used so as to obtain "projectile force," while when several larger wires are used the battery must consist of a single pair. A great number of experiments illustrative of the theory are described. With a battery having a single plate of zinc, of half a square foot of surface, he made a magnet lift a weight of 750 pounds,—more than thirty-five times its own weight.

In the same year, 1831, he describes a little machine for producing continuous mechanical motion by magnetic attraction and repulsion. He considered the apparatus to be merely a philosophical toy, involving a principle which at some future time might be applied to a useful purpose.

In 1830, at the request of Professor Renwick, he commenced a series of observations to determine the magnetic intensity at Albany. This gave him occasion to investigate a subject of which the evidences had before been very conflicting, namely, the effect of the aurora upon the magnetism of the earth.

In 1831, April 19, at 6 p. m., a remarkable phenomenon was noticed, namely, an extraordinary increase in the number of vibrations of the needle, showing a corresponding increase in the magnetic intensity of the earth. Every precaution was taken that no local influence should affect the magnet, but the result was the same. About 9 o'clock in the evening a brilliant aurora commenced. The idea now occurred to him that it might be connected with the magnetic disturbance, and another observation of the magnet was therefore made. The result was the opposite of what had been anticipated, for instead of showing a continuous increase the intensity was now far below the average. An extended discussion of other results of the same sort is given, followed by an inquiry into the origin of the aurora.

The next important investigation in which Professor Henry appears is that which led to his being an independent discoverer of magneto-electricity. In the early experiments in this direction we have an interesting example of how a discovery may be long retarded through the want of correct theoretical notions. The idea entertained by the early experimenters of the present century seems to have been that since a galvanic current passing around a core of soft iron renders it magnetic, it may be expected that a magnet placed inside of a coil of wire will cause a current of electricity to pass through it. Accordingly, endeavors were made to produce this current by using powerful magnets. But since a continuous galvanic current can be employed to produce both heat and mechanical force, it follows that if it could be produced and kept up by simply inserting a permanent magnet in a coil of wire we should have a machine working without any supply of power. Since it can hardly be supposed that these experimenters would have hoped to realize the perpetual motion, the direction in which their efforts were prosecuted could have been taken only through a failure to grasp the proper principles. These principles once apprehended, it would have been obvious that either the project of producing electricity from magnetism must be given up, or the production must be accompanied by motion or change in the magnet. The latter idea being grasped, success would at once have been assured. It happened, however, that the experiments pursued in a wrong direction necessitated this motion or change, because the magnet had to be moved to get inside the coil, or magnetism had to be produced in it in commencing the experiment, thus leading to a result.

In 1831, Faraday and Henry were independently working upon the problem. The former was entirely successful in showing how a momentary electric current could be produced by changes of magnetism in a soft iron body, or by other electrical currents, before Henry published anything of his work. No question, therefore, can attach to Faraday's claim to priority, and on the system sometimes adopted no other name than his would be mentioned in a history of the subject. But a more liberal principle now prevails, and the propriety of giving due credit to the independent investigator, though he may be behindhand in publishing, is very generally acknowledged. From Professor Henry's papers it would appear that he had actually reached a similar result before Faraday's work came to his knowledge. The magnet with which electricity was to be excited was the soft iron armature of his great galvanic magnet. A piece of copper wire thirty feet long was coiled around the middle of this armature and connected with a distant galvanometer. The great magnet being suddenly excited, the north end of the needle was deflected 30 degrees to the west, indicating a current of electricity in the helix surrounding the armature. The needle soon returned to its former position, and when the plates were withdrawn from the acid moved 20 degrees to the east. The conclusions of these experiments are now too familiar to need discussion. We can only regret that the American physicist did not immediately publish his first experiments.

In this same paper Professor Henry appears as the first discoverer of an important phenomenon of electricity now known as the self-induction of the current. A vivid spark is seen when a current through a long wire of considerable resistance is suddenly broken by withdrawing the wire from the cup of mercury through which the connection is produced. The longer the conducting wire and the larger the plates of the battery, the more vivid the spark. He attributes it to the long wire becoming charged with electricity, which by its reaction on itself projects a spark when the connection is broken.* The same discovery was independently made two or three years later by Faraday, who does not appear to have noticed Henry's description of the phenomenon.

Shortly after this Professor Henry was called to the chair of natural philosophy in Princeton College. Although the duties of an American college professor seldom allow much time for original investigation, he soon resumed his electrical researches, and the first of a regular series was communicated to the American Philosophical Society in 1835. On February 6 of that year he continued the subject of the self-induction of the electric current with especial reference to the influence of a spiral conductor upon it. The series of experiments on this subject are very elaborate, but cannot be fully described without going into a series of minute details.

On November 2, 1838, he presented an extended paper on *Electro-Dynamic Induction.*† He states that since the discovery of magneto-electricity by Faraday in 1831 attention had been almost exclusively devoted to the induction of electricity from magnetism. He had therefore been engaged in reviewing and extending the purely electrical part of "Faraday's admirable discovery" in the direction indicated in the title.

Among the little known works of Professor Henry during this period are his researches upon solar radiation and the heat of the solar spots. In connection with his relative, Professor Alexander, he may be said to have commenced a branch of modern solar physics which has since grown to large proportions, by comparing the temperature of the solar spots with that of other parts of the photosphere. The first experiments were made on January 4, 1845. A very large spot was then visible upon the sun, the image of which was formed by a four-inch telescope upon a screen in a dark room. A thermopile was placed in such a position that the image of the spot and of the neighbor-

* *American Journal of Science*, Series I, volume xxii, 1832, page 408.

† *Transactions of the American Philosophical Society*, volume vi, page 308.

ing parts of the solar disk could be thrown upon it in quick succession. The result of observations extending through several days was that decidedly less heat was received from the spot than from the brilliant part of the photosphere. It is believed that it was these experiments which started Secchi on the brilliant investigations in solar physics which he carried on in subsequent years.

Among Professor Henry's latest electrical researches was his analysis of the dynamic phenomena of the Leyden jar. The one of his discoveries which he most often referred to in later years was that the discharge of a Leyden jar did not consist of a single restoration of the equilibrium, but of a rapid succession of librations back and forth, gradually diminishing to zero. This was proved by passing the discharge through a coil of wire containing needles of different degrees of magnetic force. After the discharge these needles were found to be magnetized in different directions, according to their size and hardness.

In one of his numerous communications presented to the Philosophical Society he appears as one of the inventors of the electro-chronograph. On May 30, 1843, he presented and read a communication on a new method of determining the velocity of projectiles. It was in its essential parts identical with that now generally adopted. It consisted, he says, in applying the instantaneous transmission of the electrical action to determine the time of the passage of the ball between two screens placed at a short distance from each other on the path of the projectile. For this purpose the observer is provided with a revolving cylinder, moved by clock-work at the rate of at least ten turns in a second, and of which the convex surface is divided into a hundred equal parts, each part therefore indicating in the revolution the thousandth part of a second. Close to the surface of this cylinder, which revolves horizontally, are placed two galvanometers, one at each extremity of a diameter; the needles of these being furnished at one end with a pen for making a dot with printer's ink on the revolving surface. In the appendix to the paper he proposes to dispense with the galvanometer and produce the marks by direct electro-magnetic action, as is now done in the familiar astronomical chronograph.

While at Princeton a number of researches in other branches of experimental physics were published. It is not however necessary to describe them at length, because they are most exhaustively discussed in the memoir of Mr. Taylor before referred to. Whether they pertain to the most familiar phenomena of every-day life or the most complex combinations in the laboratory, they are all marked by the qualities of the author's mind,—acuteness in cross-examining nature, a clear appreciation of the logic of science, and an enthusiasm for truth irrespective of its utilitarian results. Reserving for the future some general remarks on the scope of Professor Henry's scientific work, the qualities which it displays, and its relation to the progress of our country, we may now pass to his connection with the Smithsonian Institution.

The origin of the Smithsonian Institution is so remarkable, and many features of its early history so instructive, that it must long continue to be a theme of interest to the historian of our intellectual development. The writer may therefore be excused for touching upon a threadbare subject by repeating the story of the origin and early difficulties of this establishment. He does so the more willingly because he believes some features connected with it have not been fully brought out.

James Smithson, a private English gentleman of fortune and scientific tastes, a chemist of sufficient note to be elected a Fellow of the Royal Society, led a comparatively retired life, and died, unmarried, in 1829. He does not seem to have left any near relatives except a nephew. On opening his will it was found to be short and simple. Except an annuity to his servant, he left the nephew, for his life, the whole income from his property, and the property itself to the nephew's children should he leave any. In case of the death of the nephew without leaving a child or children, the whole property was bequeathed "*to the United States of America, to found at Washington, under the name of the Smithsonian Institution, an establishment for the increase and diffusion of knowledge among men.*"

Probably few men have ever written a clause so well fitted as this to excite a curiosity which can never be gratified. The views and motives of the writer in making this provision are involved in impenetrable obscurity. The first idea to strike a reader would be that Smithson had some especially kindly feelings toward either the United States or its form of government. But no evidence of this has ever been discovered. He is not known to have had the personal acquaintance of an American, and his tastes were supposed to have been aristocratic rather than democratic.

It would also have been supposed that the organization of an institution which was to carry his name down to posterity would have been a subject of long and careful thought, and of conversation with friends, and would have been prescribed in more definite language than that used in the will. Some note, some appended paper would certainly be found communicating his views. But nothing of the sort has ever come to light.

The next explanation to suggest itself would be that the death of his nephew without children was a contingency so remote that very little thought was given to what might happen in that event. But it is said that on the contrary Hungerford, the nephew, was unmarried and in infirm health, and that his death without children might naturally have been expected.

We thus have the curious spectacle of a retired English gentleman, probably unacquainted with a single American citizen, bequeathing the whole of his large fortune to our Government to found an establishment which was described in ten words, without a memorandum of any kind by which his intentions could be divined or the recipient of the gift guided in applying it.

Hungerford died in 1835. An amicable suit in chancery was instituted by our Government, through the Honorable Richard Rush as its agent, the defendant being the Messrs. Drummond, executors of Smithson. Although there was no contest at any point, the suit occupied three years. On May 9th, 1838, the property was adjudged to the United States, and during the next few months disposed of by Mr. Rush for about £105,000. The money was deposited in the Treasury in the following autumn.

The problem now presented to Congress was to organize the Institution described by Smithson. The writer must confess that he does not share the views of those who maintain that the intent of Smithson was too clear and definite to be mistaken, and that the difficulty which our legislators found in deciding upon a plan shows their lack of intellectual appreciation. It is very much easier to see the right solution of a problem after it is obtained than before. It ought to be a subject of gratitude rather than of criticism that it took the country eight years to reach a conclusion. The plan at length adopted was better than any of those previously proposed, and the form into which the Institution grew was still in advance of the plan which at length passed Congress.

Whatever view we may take of this point, the diversity of projects considered by Congress shows that the meaning of the will was not made clear to our legislators. First of all there was a body of strict constructionists who maintained that our Government had no power to accept a bequest of the kind, and that the money should be returned to the English Court of Chancery. One Fleischmann, an employé of the Patent Office, petitioned for the establishment of an agricultural school, and his memorial seems to have received much attention. Another memorialist prayed for the establishment of an institution for prosecuting physical experiments, and a third that the fund might be applied to the instruction of females. A vigorous effort was made by the Columbian College to obtain assistance from the fund. Mr. John Quincy Adams desired to appropriate a considerable amount to the establishment of a great astronomical observatory. Mr. F. A. Hassler, Superintendent of the Coast Survey, desired the establishment of an astronomical school before the erection of Mr. Adams's observatory. A strong move was made by Mr. Poinsett to place the whole fund at the disposal of the National Institute for the Promotion of Literature and Science. Mr. James P. Espy, the meteorologist, proposed that a portion of the fund should be devoted to meteorological observations all over the Union. Mr. Franklin Knight wished the whole fund applied to the establishment of a farm school.

After a seven years' discussion of these and other projects and combinations, the act under which the Institution was at last organized became a law in August, 1846. It provided that the business of the Institution should be conducted by a Board of Regents, who should choose a suitable person as Secretary of the Institution. It also provided for the erection of a suitable building of plain and durable materials and structure, without unnecessary ornament, for the reception of objects of natural history, a chemical laboratory, a library and gallery of art, and the necessary lecture-rooms. The Secretary had charge of the building and property of the Institution, and was also to discharge the duties of librarian and keeper of the museum, and, with the consent of the Board of Regents, to employ the necessary assistants. All the officers were removable by the Board of Regents whenever in their judgment the interests of the Institution required them to be changed.

The Board of Regents created by the act immediately commenced active operations. In December, 1846, a committee of the Board, consisting of Mr. Robert Dale Owen, Mr. Henry N. Hilliard, Professor A. D. Bache, Mr. Rufus Choate, and Mr. Pennybacker, made a report on the plan of organization. Among the recommendations of this report the qualifications desired in the Secretary are of interest to us. It was pointed out as an almost necessary condition that the Secretary should become the chief executive officer of the Institution. After some general remarks respecting the qualifications of Secretary the report proceeds:

"Your committee think it would be an advantage if a competent Secretary could be found, combining also the qualifications of a professor of the highest standing in some branch of science. If to these be added efficiency as an executive officer and a knowledge of the world, we may hope to see filling this distinguished post a man who, when brought into communication with distinguished men and societies in this and other countries, shall be capable, as representative of the Smithsonian Institution, to reflect honor on the office, not requiring to borrow distinction from it.

"Your committee will not withhold their opinion that upon the choice of this single officer, more probably than on any other act of the Board, will depend the future good name and success and usefulness of the Smithsonian Institution."

Previous to the election of Secretary the following resolution, from the same committee, was adopted by the Board:

"*Resolved*, That it is essential, for the advancement of the proper interests of the trust, that the Secretary of the Smithsonian Institution be a man possessing weight of character, and a high grade of talent; and that it is further desirable that he possess eminent scientific and general acquirements; that he be a man capable of advancing science and promoting letters by original research and effort, well qualified to act as a respected channel of communication between the Institution and scientific and literary individuals and societies in this and foreign countries; and, in a word, a man worthy to represent before the world of science and of letters the Institution over which this Board presides."

Although couched in general terms it may be supposed that these expressions had direct reference to the subject of our notice, and were meant to justify the Board in selecting a scientific investigator of so much eminence to take charge of the establishment. Professor Henry was elected on December 3, 1846, and signified his acceptance a few days later. It was a frequent remark of his in after years that he had never sought a position, and had never accepted one without fear and trembling. Of the few positions he ever accepted we might well suppose that this was the one on which he entered with most hesitation. His position at Princeton was in every respect most agreeable. His enthusiasm as a teacher could not fail to bring around him an appreciative body of pupils. He was not moved by any merely worldly ambition to seek a larger and more prominent field of activity, and was held in the highest esteem by the authorities of the college. He thus enjoyed what is almost the happiest lot of man, that of living in a community suited to his tastes and pursuits, and of being held in consideration by all with whom he came into contact. He was now to take a position around which had raged for eight years a conflict of opinion which might at any time break out anew. That all parties could be satisfied was out of the question, and his aversion to engaging in anything which would lead to controversy was so great that he would hardly have accepted had it not been for the urgent solicitation of Professor Bache. The latter pointed out to him that the proper administration of Smithson's munificent bequest was at stake, and that he, Henry, was the only man available to whom all parties could turn with the assurance that the Institution would be carried through its difficulties. This was an appeal which he could not withstand; he therefore determined at least to make the attempt, and entered upon his duties with the assurance from the college authorities that, should he fail, his position at Princeton would always be open to him, and his friends ever ready to welcome him back.

After two or three years the divergent views respecting the proper direction to be given to the activities of the Smithsonian Institution gradually began to aggregate themselves into two groups and thus to assume a partisan aspect. Many of the projects which, during the eight years of discussion, had found supporters, were entirely given up, such, for instance, as the agricultural college, a great observatory, the instruction of women, and the establishment of a school of science. But the act of Congress provided, as already stated, for a library, a museum, a gallery of art, and courses of lectures. Henry, while yielding to the necessity imposed upon the Institution of complying with the law directing the establishment of these accessories, was in the main opposed on principle to their permanent support by the Institution. The position he took was that as Smithson was a scientific investigator, the terms of his endowment should be construed in accordance with the interpretation which he himself would have put upon his words. The increase of knowledge would mean the discovery of new truths of any sort, especially the truths of nature. The only way in which an extended diffusion of increased knowledge among men at large could be effected was by publication.

The departments of exploration, research, and publication were therefore those to which Henry was most inclined to devote the energies of the Institution. While he made no factious opposition to the collection of a library, he did not consider it as increasing knowledge or contributing to that wide diffusion of it which Smithson provided for. True, it might indirectly contribute to such diffusion by giving authors the means of preparing books; but this assistance was of too local and indirect a character to justify the appropriation of a large proportion of the Smithson funds to it. Nearly the same objections applied to the museum. The objects therein preserved were the property of the Government, and the contributions to its increase would naturally come, for the most part, from Government explorations. The explorations undertaken on behalf of the Institution would naturally be only such as, from their nature, would not be undertaken by the Government, or such as were necessary to supplement the governmental collections.

That a gallery of art would neither increase nor diffuse knowledge on the plan required by Smithson hardly needed argument. It does not seem that any serious attempt was ever made to carry out this part of the project on any considerable scale. The Indian portraits which constituted the principal part of the collection of paintings were, the writer believes, the private property of Mr. Stanley, the artist.

Perhaps the project on which the Secretary looked with most disfavor was the building. The system of operations which he would have preferred required little more than a modest suite of office-rooms. The expenditure of several hundred thousand dollars on an architectural structure seemed to him an appropriation of the funds to which he could give no active encouragement. In later years one of the warnings he often gave to incipient institutions of learning was not to spend more money in bricks and mortar than was absolutely necessary for the commencement of operations, and it can hardly be doubted that his sentiments in this direction had their origin in his dissatisfaction with the large expenditure upon the Smithsonian building.

We must not be understood as saying that Henry antagonized all these objects, considered them unworthy of any support from the Smithsonian fund, or had any lack of appreciation of their intellectual value. His own culture and mental activities had been of too varied a character to admit of his forming any narrow view of the proper administration of the establishment. The general tenor of his views may be summed up in two practical propositions:

(1.) The Institution should undertake nothing which could be done by other agencies. A paper or report which would naturally find its outlet in some other channel was never to be published by the Institution. A research made for a commercial object would find plenty to engage in it without his encouragement. It was the duty of the Government to provide room for its own collections and to make them accessible to investigators, rather than to draw upon the Smithson fund for this purpose. As a natural corollary of these views the Institution should not engage in competition with other organizations in any enterprise whatever.

(2.) Objects of merely local benefit, which no one could avail himself of except by a visit to Washington, were to be regarded as of subsidiary importance, as not well fitted to carry out the views of Smithson to the wide extent he would have desired, and as properly belonging to the local authorities.

Putting both these principles together, the library, the museum, the art gallery, the courses of lectures, and the Smithsonian building were looked upon as things only temporarily undertaken by the Institution, to be turned over to other agencies whenever such could be found ready to assume the responsibility of the operations connected with them.

The affairs of the Institution went on for several years without any interruption. The general policy of the Secretary was to keep the expenditure upon those objects which he considered least germane down to the lowest limit consistent with the law and with the resolutions of the Board of Regents, hoping gradually to win the Board over to his views. Among the accessories on which he wished to retrench, the library was the only one which gave serious trouble. In the act organizing the establishment, the Regents were authorized to make an annual expenditure, not exceeding an average of $25,000, " for the gradual formation of a library composed of valuable works pertaining to all departments of human knowledge." This sum was two-thirds of the whole annual income, and had the provision been mandatory would have left little means for active operations. At a meeting of the Board the day after the election of Professor Henry the sum of $20,000 had been appropriated for the purchase of books and the fitting up of the library. Amendments reducing the sum to $12,000 and $15,000 were successively voted down. At another meeting a more definite plan of operations was agreed upon, to take effect after the completion of the building. This was a compromise, under which one-half of the annual income should be devoted to the library, the museum and the gallery of art, and one-half to the transactions, reports, publications, lectures, and original researches. The library project thus commenced as the leading feature of the Institution. It was greatly strengthened by the character of the assistant whom Professor Henry called to its charge, Mr. C. C. Jewett, formerly librarian of Brown University, a gentleman whose high character and professional ability marked him as well fitted to undertake the work of collecting and arranging a great library. Mr. Jewett very naturally desired to expend the full admissible amount upon his department, and thus a difference gradually arose between him and his chief, which widened as the building approached completion. He began to assert his claims to an extent which met the strong disapproval of the Secretary, and in 1854 the difference culminated in an appeal to the Board of Regents.

The question was first brought before the Board in the form of a resolution respecting the proper division of the fund. In April, 1854, the executive committee recommended an appropriation in which only $6,000 was devoted to the library, more than half of which was for the salary of librarian and assistants. The appropriation for the purchase of books was only $1,800. In presenting this recommendation the committee say that they have not recommended an equal distribution between the active operations, on the one hand, and the library, museum, &c., on the other, because the compromise resolutions which required such equality of distribution do not go into effect until after the completion of the building.

This reduction was opposed by the other party on both legal and political grounds. Two members of the Board presented resolutions relative to the distribution of the income, which were referred to a subcommittee. This committee, through Hon. J. A. Pearce, its chairman, made a very elaborate report on May 25th following, reviewing the whole subject at great length, reciting what the Institution had done, and justifying the small appropriation for the library. The report closed with resolutions repealing the compromise arrangement, and leaving the apportionment among the different objects to the judgment of the Regents.

In the meantime the difference between the Secretary and the librarian reached a stage at which the further coöperation of both in the affairs of the Institution was no longer practicable. The Secretary made known his intention of removing the librarian, taking the ground that while the Board of Regents had power to remove either the Secretary or his assistants, the Secretary himself could remove the latter without reference to the Board. A resolution to this effect was introduced by Mr. James M. Mason, of Virginia. The question was, in principle, the same which has been raised from time to time since the foundation of our Government relative to the general power of superior officers over their subordinates in cases where the law makes no express provision. Under the terms of the organic act the Secretary and the Board of Regents, so far as the assistants were concerned, stood in nearly the same relation to each other that the President and Senate stand under the National Constitution. The Secretary, as executive, had the power of appointment with the consent of the Board of Regents, but the law was silent on the subject of removal. Mr. Mason's resolution, after several amendments had been voted down, was adopted by a vote of 6 to 4, and the position of the Secretary as the responsible head of the Institution was thus fully defined.

It would, however, appear that Mr. Jewett continued his efforts to secure a larger appropriation for the library than the Secretary or the executive committee considered desirable, and carried his opposition to such a point that the Secretary removed him from office on the 12th of January following.

The resolution of the executive committee repealing the compromise and leaving future annual apportionments to the judgment of the Regents was then passed by a vote of 9 to 5. A further resolution to the effect that a compliance in good faith with the letter and spirit of the charter required a large proportion of the income of the Institution to be appropriated for a library was lost.

Mr. Rufus Choate, who had been the most active supporter of Mr. Jewett and the library scheme, now resigned his position as Regent, and accompanied his resignation with a letter addressed to the Senate and House of Representatives, stating his reasons for the course he had taken, and expressing the opinion that the Smithsonian fund was being managed on a system not in accordance with the provisions of the organic act. In the Senate the subject was referred to the Committee on the Judiciary, which made a unanimous report in favor of the majority of the Board of Regents. In the House there was a more serious contest. Mr. Choate's letter was referred to a select committee of five, appointed to inquire and report to the House whether the Smithsonian Institution had been managed and its funds expended in accordance with law, and whether any additional legislation was necessary. After a careful examination, extending through a period of six weeks, the committee seems to have been unable to agree upon a report. Two reports were, in fact, made. One, signed only by Mr. Upham, the chairman, took ground against the power of removal by the Secretary of the Institution, and against the restriction of the increase of the library as contemplated. Another very elaborate report, signed by two members, sustained the Secretary and the majority of the Board. The remaining two members of the committee signed neither report;

nor did either report propose any action on the part of Congress except the payment of the clerk of the committee.

The contest which had been going on for a period of seventeen years thus ended in a complete vindication of Professor Henry and the position he had assumed. During the remainder of his life he had the great satisfaction of feeling that he was held in constantly increasing esteem both by the Regents and the public.*

In January, 1865, an event occurred which, though an almost irreparable calamity, tended materially toward the appropriation of the Smithsonian income toward those objects which the Secretary thought most proper. A considerable portion of the upper story of the main building and a part of the lower story were burned. The incipient art gallery, the chemical laboratory, and the lecture-room were all involved in the destruction. Happily the library and the museum remained nearly intact. An opportunity thus offered itself to have some of the trusts imposed upon the fund undertaken by other agencies. The Library of Congress was rapidly growing into a great national institution, so that there was no longer any sound reason for collecting a separate Smithsonian library. An act was therefore passed by Congress providing for the deposit of the Smithsonian books in the Library of Congress, so that all could be consolidated together and the Institution at the same time be relieved from their care. The necessity for reconstructing the art gallery was obviated by the prospective establishment of the Corcoran Art Gallery in a neighboring part of the city. The erection of Lincoln Hall and the establishment of courses of lectures, sometimes of a high intellectual character, by the Young Men's Christian Association, did away with the necessity of reconstructing the lecture-room. The principal immediate drawback was that the building had to be reconstructed at the expense of the Smithsonian fund, although Professor Henry was not entirely satisfied that so large a building was necessary for the Institution.

The only serious burden which remained upon the Institution was the National Museum; but the expense of its support was now undertaken by the Government, and it therefore ceased to

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* As an expression of Professor Henry's views in his own language we append to this address an extract from his examination before the English Government Scientific Commission.

be a charge upon the Smithsonian fund except in this indirect way that the building which housed it had been paid for out of that fund. No advantage would therefore have been gained by removing the museum unless the building was purchased by the Government. The Secretary was therefore desirous of effecting such a sale, but his views do not appear to have met with the entire concurrence of the Board of Regents. The latter were not unnaturally averse to seeing the Institution surrender its imposing habitation and the associations which clustered around it. A very natural compromise would have been for the Government to pay the Institution a suitable moderate rent for those portions of the building devoted to the care of Government property, but it does not appear that this measure was ever proposed.

The position of the Smithsonian building in the public grounds led Professor Henry to take an active interest in measures for the improvement of the city. Among his latest efforts in this direction were those made with the object of having the old canal which bounded the Mall filled up. Some of us may remember a witty argument with which he urged this measure upon the Board of Public Works. "The great inefficiency of the Smithsonian had been said by its opponents to be illustrated by the fact that, although formed to diffuse knowledge over the whole world, it had not diffused knowledge enough among the local authorities where it was situated to make them see the necessity of abating the pestilential nuisance of this obsolete canal." The work of filling up was immediately commenced by the board to which the argument was addressed.

The following extract from one of Professor Henry's early journals will be of interest as showing the character of his early efforts for the improvement of the Smithsonian grounds:

"NOVEMBER 25, 1850.

"Occupied this morning examining the public grounds between the Capitol and the Monument. I have been impressed since my connection with the Smithsonian Institution with the importance of improving the public grounds on which the Smithsonian is placed in accordance with a general plan, and I have taken every opportunity of expatiating on the capacity of the Mall to be made one of the most beautiful drives in the world. My enthusiasm on this point was much dampened a few months ago, when it was proposed to place the Botanic Garden on the Mall near the Smithsonian. The site was chosen and, as I supposed, all things settled, when to my surpise some influence at once changed the location.

"My interest in the project was again awakened by a movement on the part of Mr. Corcoran. An appropriation was made to improve the grounds around the President's House. Mr. Corcoran was interested in the square opposite his residence. He requested me to go with him to the President to ask him to interfere. We called on the President, who manifested an interest in the subject but said he had no power to act, but if we would show him the authority he would do what he could to forward the object. On this assurance Mr. Corcoran and myself left the President, and I was requested to search for the law authorizing the action of the President. For this purpose I called upon Peter Force, who, after a search of some time, found the law, gave me a copy, which I afterwards presented to the President. The same evening I called a meeting at the office of the mayor, of Mr. Mudd, the commissioner of public buildings, and the mayor. After some conversation it was at length concluded to send for some competent landscape gardener to give a general plan of the improvements, and, on the suggestion of the mayor, it was resolved to request the President to direct that Mr. Downing, from Newburgh, be requested to examine the grounds and report a plan of improvement. We (the mayor, Mr. Mudd, and myself) called next day on the President, presented the matter, and received from him the sanction for writing to Mr. Downing. A few days after this I started for New Jersey and was absent several days, and when I returned I found that nothing had been done,—Mr. Downing had not been written to. I therefore drew up a form of a letter of invitation in accordance with my views of the manner in which the invitation should be worded, and sent this to the commissioner. This letter was sent, and in conformity with this invitation, Mr. Downing has come on. I called with Mr. Downing on the President, who gave us a very pleasant reception and entered with much interest into the plans of Mr. Downing. This morning Mr. Mudd, Mr. Downing, and myself have examined all the ground between the Capitol and the river, and found it admirably adapted to the formation of a landscape garden and a drive."

The administration of the Smithsonian Institution does not appear to have been compatible with the continuance of the experimental researches in which our colleague was so eminently successful during the earlier years of his life. The fact is that the general science of electricity was passing almost beyond the experimental and into the mathematical stage, so that little of real value could be effected by mere experimentation without reference to purely mathematical theories. But it would be altogether a mistake to suppose that his scientific activity was diminished or that his contributions to knowledge were confined to his earlier days. The talent which had before been directed to investigations of a purely scientific character (understanding by this term such as were designed only to improve the theories of natural phenomena) was now turned to practical application of scientific principles. Whether such applications are less worthy of the investigator than the advancement of purely theoretical notions, we shall not attempt to discuss, but shall only remark that our colleague brought into his new field that same unselfish devotion to the intellectual interests of mankind which marks the purely scientific investigator. Whatever utilitarian objects he may have aimed at, they had no personal reference to himself. He never engaged in an investigation or an enterprise which was to put a dollar into his own pocket, but aimed only at the general good of the world.

One of the earliest of his new enterprises was that of receiving notices of the weather by telegraph and exhibiting them upon a map, thus laying the foundation of our present meteorological system. In 1847 he called the attention of the Board of Regents to the facilities which lines of telegraph would afford for warning observers to be on the watch for the approach of a storm. As a part of the system of meteorology, the telegraph was to be employed in the investigation of atmospheric phenomena. The advantage to agriculture and commerce to be derived from a knowledge of the approach of a storm was recommended as a subject deserving the attention of Government. About 1850 the plan of mapping the weather was instituted. Many of us remember the large maps of the country suspended in the entrance to the Institution, on which the state of the weather in different regions was indicated by movable signs. This system continued until 1861, when the breaking out of the civil war prevented its further continuance.*

After the close of the war a renewal of the system was proposed and some efforts made for the attainment of this object. But with this, as with every other enterprise, Professor Henry would never go on with it after any one else was found ready to take it up. In 1869 our colleague, Professor Abbe, commenced the issue of regular weather bulletins from the Cincinnati Observatory, showing the state of the weather at a number of telegraphic stations, followed by a brief forecast of the weather which would probably be experienced at Cincinnati during the next twenty-four hours. About the same time provision was made by Congress for the national system now so thoroughly organized by the Chief Signal Officer of the Army. This system received the cordial support of Professor Henry, who gave every facility at the disposal of the Institution to General Myer for the completion of the organization, and, indeed, turned over the whole practical part of the subject to him.

Among the services of Professor Henry outside of the field of pure science and of the administration of the Smithsonian Institution the first place is due to those rendered in connection with the Light-House Board. This Board was organized by act of Congress in 1852 to discharge all administrative duties relating to the light-house establishment on the American coasts. The duties assigned to Professor Henry in this connection included experiments of all kinds pertaining to lights and signals. The illuminating power of various oils was made the subject of exact photometric experiments, and large sums were thus saved to the Government by the adoption of those illuminators which gave most light in proportion to cost. The necessity of fog signals led to what are, for our present purpose, the most important researches in this connection, namely, his investigations into the phenomena of sound. Acoustics had always been one of his favorite subjects. As early as 1856 he published a carefully prepared paper on the acoustics of public buildings, and he frequently criticised the inattention of architects to this subject. His regular investigations of sound in connection with the Light-

*See Historical Notes on the System of Weather Telegraphy, by Cleveland Abbe. *American Journal of Science and Arts*, volume ii, 1871, page 81.

House Board were commenced in 1865. It had long been known that the audibility of sounds at considerable distances, and especially at sea, varies in a manner which has seemed quite unaccountable. There were numerous instances of a sound not becoming audible until the hearer was immediately in its neighborhood, and others of its being audible at extraordinary distances. Very often a sound was audible at a great distance and was lost as the hearer approached its source. The frequency of fogs on our eastern coasts and the important part played by sound signals in warning vessels of danger rendered it necessary to investigate the whole theory of the subject.

One of the first conclusions reached related to the influence of reflectors and of intervening obstacles. That a sound in the focus of a parabolic reflector is thrown forward and intensified in the manner of light has long been a well-known fact. The logical consequence of this is that the sound is cut off behind such a reflector, so that at short distances it is many times louder in front of the reflector than behind it. In the case of light, which moves in right lines, it is well known that such an increased volume of light thrown in one direction will go on indefinitely. But in the case of sound the law was found to be altogether different—the farther the observer went away from the source, the less the influence of the reflector, and at the distance of two or three miles the latter was without effect,—the sound being about equally audible in whatever direction the reflector might be turned. Another important discovery, made the following year, was that when a sound was moving against the wind it might be heard at an elevation when it was inaudible near the surface of the water.

These observations were continued from time to time during the summer season until 1877. They resulted in collecting an immense mass of facts, including many curious abnormal phenomena, descriptions of which are found in the annual reports of the Light-House Board. Our president was extremely cautious in formulating theories of the subject, and had no ambition of associating his name with a generalization which future researches might disprove. The result of his observations however was to show that there were none of these curious phenomena which might not be accounted for by a species of refraction arising from varying atmospheric currents. The possible effects of this cause had been pointed out by Professor Stokes, of England, in 1857, and the views of the latter seem to have been adopted by Henry. One of the generalizations is very clearly explained on this theory: A current of air is more rapid at a short height above the water than at its immediate surface. If a sound-wave is moving with such a current its upper part will be carried forward more rapidly than its lower part; its front will thus be presented downward and it will tend to strike the water. If moving in an opposite direction against the wind, the greater velocity of the latter above the water will cause the upper part of the sound-wave to be retarded. The wave will thus be thrown upward, and the course of the sound will be a curved line convex to the water. Thus an observer at the surface may be in a region of comparative silence, when by ascending a few yards he will reach the region of sound vibration. A corresponding effect would be produced by a difference in the motions of two contiguous bodies of air, whether the line of change was vertical or horizontal. As we know very well that the motion of the air is by no means uniform, and that eddies, gusts, and whiffs prevail nearly everywhere, it is to be expected that sound will not always move uniformly in a direct line, but will be turned from its direct course by the sort of refraction that we have described. It is however impossible to prove by observation that this is the only cause of the abnormal phenomena referred to, because the exact velocity of local currents within a space over which the sound extends cannot be a subject of observation. Professor Henry was however disposed to claim that, having a sufficiently general known cause to account for the phenomena, it was not philosophical to assume other causes in the absence of decisive proof.

It was at the light-house station in the month of December, 1877, that Professor Henry noticed the first symptom of the disorder which terminated his life a few months later. After passing a restless and uncomfortable night, he arose in the morning, finding his hand partially paralyzed. A neighboring physician, being sent for, made a prognosis of a very serious character. A more detailed subsequent examination by two members of our Academy led to the conclusion that he was affected with an incipient nephritis. Although no prospect of recovery could be held out, it was hoped that the progress of the disease would be so slow that, with his healthy constitution, he might still endure for a considerable period. This hope however rapidly faded. During the winter the disease assumed so decided a form as to show that his active work was done and that we could have him with us but a few months longer. But beyond a cessation of his active administrative duties there was no change in his daily life. He received his friends, discussed scientific matters, and took the most active interest in the affairs of the world so long as his strength held out. It was a source of great consolation to his family and friends that his intellect was not clouded nor his nervous system shattered by the disease. One of the impressive recollections of the writer's life is that of an interview with him the day before his death, when he was sustained only by the most powerful restoratives. He was at first in a state of slumber, but, on opening his eyes, among the first questions he asked was whether the transit of Mercury had been successfully observed and the appropriation for observing the coming total eclipse secured. He was then gradually sinking, and died at noon on May 13, 1878.

A mere sketch, like the foregoing, of the lines of activity followed out by our late President, gives no adequate idea either of his mental force or of his public services. The contributions to science of an American of the last few generations afford an entirely insufficient standard of judgment, though it is a standard which writers are prone to adopt as if it were the only one. We are apt to forget that science is a plant of cultivation which rarely or never flourishes in a state of isolation, and reaches full fruition only when it can absorb into its own growth the fertile ideas of many associated minds. Leaving out a few powerful intellects who started our modern system of investigating nature, a high development of the scientific spirit has been attained only by a communion of ideas through the medium of academies, institutions, and journals. We may pronounce it an entire illusion to suppose that a professor in one of our ordinary American colleges, without personal contact with men engaged in similar pursuits, and without access to the publications in which foreign investigators publish their researches, can permanently take a leading position in any branch of investigation. If it shall appear that Henry's contributions to electricity were less numerous and brilliant than those of Faraday, let us consider not simply the immensely wider field of Henry's intellectual and public activity, but the different situations of the two men. The one occupied the focus of the intellectual metropolis of the world, commanding at pleasure every sort of apparatus which money could purchase or art produce, and was surrounded by an admiring crowd of the *élite* of society, eagerly hearing of his every discovery and listening attentively to all his utterances. The other was, during his early prime, an overworked instructor, almost out of the reach of the great treasures of foreign scientific literature, and with none of the advantages enjoyed by his great competitor.

Another circumstance not to be lost sight of is that Henry, in obedience to one of the great principles of his life, voluntarily relinquished to others each field of investigation at the very time when he had it so far cultivated that it might yield him fame and profit. It is an unfortunate fact that the world, in awarding its laurels, is prone to overlook the sometimes long list of those whose labors have rendered a result possible, and to remember only the one who gave the finishing stroke, or applied previously known principles to some useful result. There are few investigators to whom the criterion in question would do less justice than to the subject of our notice. In his unselfish devotion to knowledge he sowed that others might reap on the broad humanitarian ground that a valuable harvest would be sure to find a reaper while the seed might wait in vain for a sower. Had this been done solely in his individual character we should have looked upon his course with admiration; but in bringing the principle into the administration of the Smithsonian Institution he avoided a danger and rendered a benefit for which we cannot be too grateful. To this principle is due the fact that the Institution never appeared as a competitor, seeking an advantage for itself, but always as the active coöperator in every enterprise tending to carry out the object prescribed by its founder.

Notwithstanding a uniform adherence to this course through his whole life, it would be difficult to find a physicist of our time whose researches cover more ground than his do. Any adequate analysis of his published papers and notices would transcend the limits of the present memoir. Besides his electrical researches, they include meteorology in almost all its phases, the physical geography of his native State, terrestrial magnetism, capillarity, molecular physics, observations of meteors, phosphorescence, solar physics, protection from lightning, observations of the aurora, the radiation of heat, the strength of building materials, experiments on an alleged spontaneous separation of alcohol and water, aeronautics, the ventilation of buildings, the phenomena of sound, and various other subjects hardly admitting of classification.

Notwithstanding his literary productiveness, he rarely if ever wrote a paper to yield him the honorarium of a magazine contributor. Nor did he ever seek a source of income beyond the modest salary paid him for administering the Smithsonian Institution. This sufficed, not only to satisfy the wants of a simple mode of life, but, with the aid of the accommodations allowed him in the building, to dispense a hospitality to a wide circle of friends and admirers as pleasant to the recipients as if it had won the title of princely. Although not drawing a salary from the Government, and entitled therefore to compensation for any services rendered, his numerous public services were entirely gratuitous. It must however be said to the credit of our Government that after his death Congress voted his family a small compensation for his twenty-five years of administrative service in the offices of member and president of the Light-House Board.

One of his interesting traits of character, and one which powerfully tended to make the Smithsonian Institution popular and useful, was a certain intellectual philanthropy which showed itself in ceaseless efforts to make others enjoy the same wide views of nature which he himself did. He was accessible to a fault, and ever ready to persuade any honest propounder of a new theory that he was wrong. The only subject on which the writer ever had to express to him strong dissent from his views was that of the practicability of convincing "universe-makers" of their errors. They always answered with opposing arguments, generally in a tone of arrogance or querulousness which deterred even the modest Henry from replying further; but in spite of oft-repeated failure he still considered it a duty to do what he could toward imbuing the next one of the class who addressed him with correct notions of scientific theories.

It is hardly necessary to say that in Professor Henry's mental composition were included a breadth of intellect, clearness of philosophic insight, and strength of judgment, without which he could never have carried out the difficult task which his official position imposed upon him. His mental fiber was well seen in the stand which he took against the delusions of spiritualism. On no subject was he more decided than on that of the impossibility and absurdity of the pseudo-miracles of the mediums, who seemed to him to claim no less a power than that of overruling the laws of nature. An intellectual person yielding credence to their pretensions seemed to him to be in great danger of insanity. An old and respected friend, who had held a prominent position in the Government service, in speaking to him on the subject, once described how he had actually seen a spiritual medium rise in the air and waft himself out of the window. "Judge," answered the Professor, "you never saw that, and, if you think you did, you are in a dangerous mental condition. If you do not give this delusion up you will be in the insane asylum before you know it. As a loving friend I beseech you to take warning of what I say, and to reflect that what you think you saw is a mental delusion which requires the most careful treatment."

He used frequently to relate a curious circumstance as an illustration of the character of this "spiritual" legerdemain. A noted spiritualist had visited Washington during Mr. Lincoln's administration, and held several seances with the President himself. The latter was extremely desirous that Professor Henry should see the medium, and give his opinion as to how he performed his wonderful feats. Although Henry generally avoided all contact with such men, he consented to receive him at the Smithsonian Institution. Among the acts proposed was that of making sounds in various quarters of the room. This was something which the keen senses and ready experimental faculty of the Professor were well qualified to investigate. He turned his head in various positions while the sounds were being emitted. He then turned toward the man with the utmost firmness and said, "I do not know how you make the sounds, but this I perceive very clearly: they do not come from the room but from your person." It was in vain that the operator protested that they did not, and that he had no knowledge how they were produced. The keen ear of his examiner could not be deceived.

Some time afterward the Professor was traveling in the east, and took a seat in a railway car beside a young man who, finding who his companion was, entered into conversation with him, and informed him that he was a maker of telegraph instruments. His advances were received in so friendly a manner that he went further yet, and confided to him that his ingenuity had been called into requisition by spiritual mediums, to whom he furnished the apparatus necessary for the manifestations. Henry asked him by what mediums he had been thus engaged, and was interested to find that among them was the very man he had met at the Smithsonian. The sounds which the medium had emitted were then described to the young man, who in reply stated that the apparatus had been constructed by himself, and explained its structure and working. It was fastened around the muscular part of the upper arm, and so devised that the sounds would be produced by a simple action of the muscle, unaccompanied by any motion of the joints of the arm, and therefore entirely invisible to a bystander.

A trait of Professor Henry's character which contributed powerfully to his success and usefulness was the many-sidedness of both his intellect and his taste. The great development of the imaginative and æsthetic faculties which led to the precocious dramatic activity of his boyhood made itself felt throughout his life. Although he did not seek to beautify his public addresses or communications with ornaments drawn from foreign sources, he was always ready with an apt quotation to clothe a sentiment. Apart from all intellectual and scientific claims, American science could not have desired a more fitting representative and leader at the National Capital, or found one whose personality afforded so little ground for adverse criticism. His principles kept him outside of all competition, jealousies, and cross-purposes, and his purity of motive gave his recommendations a force, founded on the assurance of their entire disinterestedness, which they otherwise could not have commanded. If he had eccentricities or prejudices they were those of the philosopher. The mental qualities so well fitted to secure the affection as well as the respect of all with whom he became intimately acquainted were supplemented by a healthy constitution, an attractive person, and a commanding, yet modest, presence, finely calculated to win confidence.

In conclusion, we believe that we but feebly express the sentiment of every member of the Academy in saying that our late President will be entitled to the gratitude of posterity as the leader of that intellectual band of the last generation, to whom is due the great advance in the national appreciation of scientific research which we have witnessed during the last thirty years, and that the society of which he would not be an ornament is still beyond our intellectual vision.

Supplemental Note.

The following statement by Professor Henry was made at the request of the English Government Scientific Commission, June 28, 1870, during his visit to London. To the request that he would give the Commission a general idea of the character of the Smithsonian Institution, Professor Henry replied :

" There was at first a great diversity of opinion as to the manner in which the income should be applied to realize the design of the testator, as expressed in the brief but comprehensive terms of the bequest. The distinction at that time between an Institution for the advancement of knowledge by the discovery of new truths, and one for the teaching of the knowledge already in existence, was not so generally recognized as it is at present, and Congress, after several years of delay, placed the expenditures of the income under the care of a Board of Regents, and directed that they should make provision, by the erection of a building and otherwise, for the formation of a library, a museum, and a gallery. It also gave fifty acres of unimproved ground, surrounding the site for the building, with indications that it should be planted with trees. Afterward, however, though not without much opposition, it was concluded by the directors that those objects, although very important in themselves, were too local in their influence to come up to the liberal spirit of the bequest, which was intended not merely to benefit the citizens of Washington, nor even exclusively those of the United States, but mankind in general; and that the efforts of the directors should be to induce Congress to make a separate appropriation, from the public treasury, for the support of the objects just mentioned, and to devote, as far as possible, the income of the Smithsonian fund to the direct increase and diffusion of knowledge, by promoting original researches, and by distributing accounts of the results of these to every part of the civilized world. In this the directors have been in a great measure successful, though time and much persevering labor have been required to produce a change in the policy originally contemplated. A large portion of the income of the funds has been expended on the building. A library, principally consisting of nearly a full series of the proceedings and transactions of the existing learned societies of the world, has been accumulated, the expense of the care of which has absorbed another portion of the income; a museum has been collected, consisting principally of specimens to illustrate the natural history and ethnology of America, and also a collection of engravings and plaster casts to meet the original requirements of Congress as to a gallery of art; but experience has abundantly proved that any one of the specified objects, if properly sustained, would soon absorb all the income of the bequest, and vindicated the policy of transferring the support of them to other funds. In accordance with this, Congress was first induced to take charge of the grounds and take the steps necessary for their improvement. It next took charge of the books which had been collected and incorporated them with the national library, giving the Institution and its collaborators the free use of the books of both collections. By this transfer the Institution is saved, in the expense of binding, cataloguing, and attendance, nearly $10,000 annually, while it has the same use of its books as before the arrangement was made. Again, the Agricultural Department has taken charge of the plants of the Institution, and the osteological specimens have been transferred to the Army Medical Museum. Furthermore, a wealthy citizen of Washington has made a large appropriation of money to establish and support a gallery of art, and it is proposed to transfer to this the articles which the Institution has accumulated in the line of art. The object of this policy is to establish at Washington a collection of objects of nature and art, without trenching on the Smithsonian fund, which shall be worthy the capital of the nation. As a step toward this desirable end, Congress, at its present session, has appropriated $10,000 towards the support of the museum, under the care of the Institution, and also $10,000 for the commencement of the fitting up of the upper story of the Smithson building for the better display of the collections. The $10,000 for the care of the museum will, for the present, be an annual appropriation."

Q. "What does the building itself represent?" A. "Externally a Norman castle, and it has cost a very large sum. Unfortunately, architecture is frequently in antagonism with science, and, too often, when an architect gets his hand into the purse of an establishment everything else must stand aside. Much trouble has resulted from this building; it has been a source of constant anxiety and expense,—the cost having greatly exceeded the original estimate."

Q. "What was the original object of the building?" A. "It was intended to accommodate a library, a museum, and a gallery of art, but, inasmuch as the Institution has turned over the library and the gallery of art to other establishments, the building will now be devoted entirely to the museum. The upper part of it was burnt, and it remains unfinished; and if Congress would accept the building as a gift, allowing one of the wings for the use of the Institution, and devoting the main portion to the museum, it would be a gain to the Institution."

LIST OF THE SCIENTIFIC PAPERS OF JOSEPH HENRY.*

1825. On the Production of Cold by the Rarefaction of Air, accompanied with Experiments. (Presented March 2.) Abstract, *Trans. Albany Institute*, vol. i, part ii, p. 36.

1827. On some Modifications of the Electro-magnetic Apparatus. (Read Oct. 10.) *Trans. Albany Inst.*, vol. i, pp. 22–24.

1829. Topographical Sketch of the State of New York; designed chiefly to show the General Elevations and Depressions of its Surface. (Read Oct. 28.) *Trans. Albany Inst.*, vol. i, pp. 87–112.

1829. First Abstract of Meteorological Records of the State of New York for 1828. (In conjunction with Dr. T. Romeyn Beck.) *Annual Report of Regents of University*, to the Legislature of New York.—Albany, 1829.

1829. On the Mean Temperature of Twenty-seven different Places in the State of New York, for 1828. (In conjunction with Dr. T. Romeyn Beck.) Brewster's *Edinburgh Jour. Science*, Oct., 1829, vol. i, n. s., pp. 249–259.

* From the Memorial of Joseph Henry, published by order of Congress, 1880.

1830. Second Abstract of Meteorological Records of the State of New York for 1829. (In conjunction with Dr. T. Romeyn Beck.) *Annual Report of Regents of University*, to the Legislature of New York.—Albany, 1830.

1831. On the Application of the Principle of the Galvanic Multiplier to Electro-magnetic Apparatus, and also to the development of great Magnetic power in soft iron, with small Galvanic Elements. Silliman's *American Jour. Science*, Jan., 1831, vol. xix, pp. 400–408. *Jour. of Roy. Institution of Gr. Brit.*, May, 1831, vol. i, pp. 609, 610.

1831. Tabular Statement of the Latitudes, Longitudes, and Elevations of 42 Meteorological Stations in New York. *Annual Report Regents of University*, to Legislature N. Y. 1831.

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1832. On a Disturbance of the Earth's Magnetism in connection with the appearance of an Aurora as observed at Albany on the 19th of April, 1831. (Communicated to the Albany Institute, Jan. 26, 1832.) *Report of Regents of University*, to the Legislature of New York.—Albany, 1832. Silliman's *Am. Jour. Sci.*, July, 1832, vol. xxii, pp. 143–155.

1832. Fourth Abstract of Meteorological Records of the State of New York for 1831. (In conjunction with Dr. T. Romeyn Beck.) *Annual Report of Regents of University*, to the Legislature of New York.—Albany, 1831.

1832. On the Production of Currents and Sparks of Electricity from Magnetism. Silliman's *Am. Jour. Sci.*, July, 1832, vol. xxii, pp. 403–408.

1832. On the effect of a long and helical wire in increasing the intensity of a galvanic current from a single element. (Conclusion of preceding paper.) Silliman's *Am. Jour. Sci.*, July, 1832, vol. xxii, p. 408. Becquerel's *Traité expérimental de l'Électricité*, etc., 1837, vol. v, pp. 231, 232.

1833. Fifth Abstract of Meteorological Records of the State of New York for 1832. (In conjunction with Dr. T. Romeyn Beck.) *Annual Report of Regents of University*, to the Legislature of New York.—Albany, 1833.

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1835. Contributions to Electricity and Magnetism. No. II. On the influence of a spiral conductor in increasing the intensity of electricity from a galvanic arrangement of a single pair, etc. (Read Feb. 6.) *Trans. Amer. Phil. Soc.*, vol. v, n. s., pp. 223–232. Sturgeon's *Annals of Electricity*, etc., vol. i, pp. 282–290. Taylor's *Scientific Memoirs*, vol. i, pp. 540–547.

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1838. A letter on the production directly from ordinary Electricity of Currents by Induction, analogous to those obtained from Galvanism. (Read to Philosoph. Society, May 4.) *Proceedings Am. Phil. Soc.*, vol. i, p. 14.

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S. Alexander, and J. P. Espy.) *Proceedings Am. Phil. Soc.*, Dec. 21, vol. i, pp. 162, 163. Silliman's *Am. Jour. Sci.*, Oct., 1840, vol. xxxix, pp. 372, 373.

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1845. On the discharge of electricity through a long wire, etc. (Read Nov. 7.) *Proceedings Am. Phil. Soc.*, vol. iv, pp. 208, 209.

1846. Repetition of Faraday's Experiment on the Polarization of Liquids under the influence of a galvanic current. (Read Jan. 16.) *Proceedings Am. Phil. Soc.*, vol. iv, pp. 229, 230.

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1846. Report on the action of Electricity on the Telegraph Wires, and Telegraph Poles struck by Lightning. (Read June 19.) *Proceedings Am. Phil. Soc.*, vol. iv, pp. 260–268. Silliman's *Am. Jour. Sci.*, 1847, vol. iii, pp. 25–32. *L. E. D. Phil. Mag.*, 1847, vol. xxx, pp. 186–194. *Agricultural Report*, Comm'r Pats., 1859, pp. 509–511.

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1855. On the mode of Testing Building Materials, etc. *Proceedings Am. Assoc.*, August, 1855, pp. 102–112. Silliman's *Am. Jour. Sci.*, July, 1856, vol. xxii, pp. 30–38. *Smithsonian Report*, 1856, pp. 303–310.

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1855. Remarks on Rainfall at varying elevations. *Smithsonian Report*, 1855, pp. 213, 214.

1855. Directions for Meteorological Observations. (In conjunction with Professor A. Guyot.) *Smithsonian Report*, 1855, pp. 215–244.

1855. Circular of Inquiries relative to Earthquakes. *Smithsonian Report*, 1855, p. 245.

1855. Instructions for Observations of the Aurora. *Smithsonian Report*, 1855, pp. 247–250.

1855. On Green's Standard Barometer for the Smithsonian Institution. *Smithsonian Report*, 1855, pp. 251–258.

1855. Circular of Instructions on Registering the periodical phenomena of animal and vegetable life. *Smithsonian Report*, 1855, pp. 259–263.

1855. Meteorology in its connection with Agriculture, Part I. *Agricultural Report* of Comm'r Pats., 1855, pp. 357–394.

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1860. Circular to Officers of Hudson's Bay Company. (April 20.) *Smithsonian Miscell. Collections*, No. 137, vol. viii, pp. 1–4.

1860. Description of Smithsonian Anemometer. *Smithsonian Report*, 1860, pp. 414–416.

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1863. Letter to Orlando Meads, Chairman of Committee of Trustees, etc., on the semi-centennial celebration of the Albany Academy. (Dated June 23.) *Proceedings on Semi-Centennial Anniversary*, etc., pp. 66, 67.

1863. Introduction to Memoir by Professor J. Plateau. On the Figures of Equilibrium of a Liquid Mass., etc. *Smithsonian Report*, 1863, pp. 207, 208.

1864. On Materials for Combustion in Lamps of Light-houses. (Read Jan. 12, before the National Academy of Sciences.) [Not published in Proceedings.]

1865. Report relative to the Fire at the Smithsonian Institution, occurring Jan. 24, 1865. (In conjunction with Mayor Richard Wallach.) Presented to the Regents February, 1865. *Smithsonian Report*, 1864, pp. 117–120.

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1865. Remarks on the Meteorology of the United States. *Smithsonian Report*, 1865, pp. 50–59.

1865. Remarks on Ventilation, especially with reference to the U. S. Capitol. *Smithsonian Report*, 1865, pp. 67, 68.

1866. Report on the Warming and Ventilating of the U. S. Capitol. (May 4.) *Exec. Doc.* No. 100, H. of Rep., 39th Cong., 1st sess., pp. 4-6.

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1866. Investigations in regard to Sound. (Read August 10, before the National Academy of Sciences.) [Not published in Proceedings.]

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1867. Notes on Atmospheric Electricity. To correspondents. *Smithsonian Report*, 1867, pp. 320-323.

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1870. Letter on a Physical Observatory. (Dec. 29.) *Smithsonian Report*, 1870, pp. 141-144.

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1871. Instructions for Observations of Thunder Storms. *Smithsonian Miscell. Collections*, No. 235, vol. x, p. 1.

1871. Circular relative to Heights. For a topographic chart of N. America. *Smithsonian Miscell. Collections*, No. 236, vol. x, p. 1.

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1872. Remarks on Cosmical Theories of Electricity and Magnetism: an Appendix to a Memoir by Professor G. B. Donati. *Smithsonian Report*, 1872, pp., 307–309.

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My Dear Mother

I have only one minute to say farewell being on board the Dover for Liverpool from whence I will write you — My plan is to return in the Boston packet of September when please God I will see you again — My wife goes with me as we have no family to seperate us — in the mean time I send you the "Souvenir" which contains an engraving said by every body to resemble her — of which you can judge in the Fall of the year — Love to family and God bless you all

your Son
J E Holbrook

Ship Dover —

BIOGRAPHICAL MEMOIR

OF

JOHN EDWARDS HOLBROOK.

1794-1871.

BY

THEODORE GILL.

READ BEFORE THE NATIONAL ACADEMY OF SCIENCES,
APRIL 22, 1903.

BIOGRAPHICAL MEMOIR OF JOHN EDWARDS HOLBROOK.

Of the early students in America of the animal kingdom, none was more disinterested in pursuit of knowledge or spent more liberally of his means than JOHN EDWARDS HOLBROOK. His attention was early directed to herpetology, and he devoted time and fortune to the cultivation of that science and to the description and pictorial delineation of the reptiles and amphibians of the United States. After he had finished his labors in that branch he transferred his attention to the sister science of ichthyology, and carried over to it the same desire for perfection and the same high ambition. He hoped to produce a work on fishes that would compare favorably with that on reptiles. Adverse circumstances, however, prevented the completion of his scheme. Under what circumstances he worked we may learn from various sources. There is a pathetic interest in the tale of his endeavors.

I.

Holbrook was of direct New England origin and of a remote English ancestry. His father and mother were respectively Silas and Mary Holbrook, and his middle name involves a reminiscence of his maternal relations. His grandfather was Daniel Holbrook, and farther back in time appears the name of the first known ancestor in New England—Thomas Holbrook. His father's early home was Wrentham, a town in Massachusetts about 27 miles from Boston and 18 from Providence; but he married Mary Edwards, of Beaufort, South Carolina, and had a residence at the latter place, spending much of his time there after his marriage. The place of Holbrook's birth was Beaufort, as was also that of his brother. According to the best authorities, he was born December 30, 1794. The obituaries published in *Harper's Weekly*, *The American Journal of Science*, the original accounts in *Appletons' Cyclopædia of American Biography*, and *Lamb's Dictionary*, and that by Prof. Louis Agassiz, all concur in giving this date. Nevertheless, in the notice in Allibone's *Critical Dictionary of English Literature* 1795 is specified, and in the special memoir by his friend and colleague of many years, Dr. T. L. Ogier, 1796 (the year of a brother's birth) is given.

When a year and a half old, the infant was taken by his parents to Wrentham. In that town he spent his childhood, and there he was taught the elements of knowledge. In due time he was sent to the near city of Providence, and went through the regular collegiate course of Brown College, graduating and receiving the degree of A. B. in 1815. He then went to Philadelphia and entered upon a routine of medical study in the University of Pennsylvania, and, after a course of three years, in 1818 received the degree of M. D. Soon afterward he went to Boston, and made a brief trial of practice with a medical friend in that city, but before long came to the determination to become still better grounded in his profession and to see more of the world. He therefore proceeded first to London and Edinburgh, and continued his medical studies in the Scottish capital for a couple of years. Afterward he proceeded to the continent, and spent about two more years in travel through France, Germany, and England, and profited by a sojourn of several months in Paris, enjoying intercourse with some of the eminent naturalists that then glorified France. Among them were Valenciennes and Duméril, the former of whom was collaborating with Cuvier in his great work on fishes, and the latter was even then preparing for his extensive work on reptiles. Professor Agassiz has expressed the opinion that "perhaps nothing in all his European journey had greater influence upon his future life than his stay in Paris, where he worked at the Jardin des Plantes, and became intimate with some of the leading scientific men of the day. He formed relations then which ended only with his life, such as his friendship with Valenciennes, with Duméril, Bibron, and others." Doubtless from them he imbibed the taste for herpetology and ichthyology and the methods of study which he later adopted.

In 1822, the young man returned to his own country and soon settled at Charleston, where he became a candidate for the practice of his profession. He was now twenty-eight years old. When he reached the age of thirty (in 1824) he coöperated with some of the leading physicians of his adopted city in the organization of "The Medical College of South Carolina" and was elected professor of anatomy. This chair he was destined to hold for over thirty years. His carriage in this situation and his personal character may be best given in the words of an early student, later a colleague in the practice of medicine as well as in the college, and a long-time friend, Dr. T. L. Ogier.

"In 1827 Dr. Holbrook married Miss Harriott Pinckney Rutledge, of South Carolina, one of the most talented and gifted women that ever gave happiness to a family or ornamented society. With a highly cultivated mind and good taste, she encouraged and assisted the Doctor in his scientific pursuits. Never were two persons better suited to each other, and never was there a happier home than theirs. Like her husband, she possessed the rare faculty of attaching warmly to her all who were brought under her influence; and her position in society, which at that time in Charleston was as refined as in any part of the world, was as enviable as the Doctor's was in his profession. Who that has ever partaken of the hospitalities of Belmont can ever forget the refinement, brilliancy, and kindness of its accomplished hostess? The calamities of the war and the death of many loved friends affected seriously her health. Her nervous system completely broke down, and she died after a short illness in Columbia, November, 1863. Having no family herself, she devoted much of her time in superintending the education of her nieces, who all retain vividly the signet stamped upon them by this refined and accomplished woman.

"Dr. Holbrook, as a practitioner, was very popular. He had the peculiar faculty of attaching warmly to him all who were brought in contact with him. There was something in his manner which was irresistible; hence his patients felt the most entire confidence in his ability to relieve their sufferings; and we all know how much this condition of the patient's mind contributes in many cases to the success of the treatment. The Doctor had some peculiarities or eccentricities, but these could be easily explained; for instance, his dislike to attend obstetric cases, or to perform any painful surgical operation. He never attended an accouchement in the whole course of his practice; and with his accurate knowledge of anatomy, he never operated if he could get any one else to do it in whom he had confidence. He would advise the operation to be performed, speak of all the details, and often assist until the operation was under way, and then quietly withdraw himself until it was over, when he would again appear, say something cheerful to the patient, and attend him afterwards with the greatest kindness and efficiency. We believe this was entirely owing to his great dislike to seeing persons suffer pain. It seemed to distress him often quite as much as it did the patient. This feeling no doubt also prevented his ever practicing midwifery, notwithstanding the many solicitations he had to do so. His manner in a sick room was gentle and kind; but, to those who did not know him well, sometimes seemed abrupt; for instance, where a patient was suffering from nausea and seemed likely to vomit the medicine he had just administered, he frequently, without any notice and regardless of bed-clothes, would throw half a tumblerful of water in his face and say, 'Take that, and do not say anything more about throwing up.' The patient would be momentarily startled, and protest against such treatment; but the nausea would pass off, the medicine be retained, and his temporary wrath against the Doctor would be changed to the feeling of gratitude and confidence. One of his eccentricities was followed by a remarkable result. A young waitingman about the house was very liable to fainting fits. In brushing flies at the table he would often exclaim, 'I am going to faint,' and would, if not assisted, fall down in a swoon. The Doctor one day, whilst dissecting the digestive apparatus of a young alligator, called this boy to hold the parts for him, so as to keep the fibers stretched. Just as the Doctor was most interested in tracing some minute muscular fibers, the boy cried out, 'I am going to faint,' and altered the position of his hand, and thus interrupted the dissection at a most important point. The Doctor immediately gave him a sharp slap on the side of his head, saying, 'Well, go faint then, and come back quickly.' The boy did as he was bid, and never fainted after this. He said he 'was cured by holding the alligator.'

"As a lecturer on anatomy, Dr. Holbrook possessed qualities which were never surpassed or very seldom equaled. With a thorough knowledge of the structure of the human body and a peculiar talent for description, he brought his knowledge of comparative anatomy to enforce and enlighten his demonstrations, which made his lectures not only instructive, but most delightful to listen to. The advanced student felt that he had been taught something beyond what his books had taught him, and the beginner that he had entered upon the study of the most wonderful and beautiful work of God; that he must bring his whole soul into his work; that it would be a crime to be a mere smatterer in this divine science, and worse to pervert it to any other than its intended purpose, the promotion of the happiness of mankind and the benefit of creation. Some of his lectures, particularly those on the brain, were not only highly instructive, but were beautiful sermons, demonstrating by correct diagrams and specimens the nervous system in the lower order of animals, from the acephalous up to the vertebrated, showing how parts were gradually added, according to the necessities of the animals, and finally to man, with the development of his great anterior cerebral lobes, not to perfect his physical qualities, but to enable him, unlike the other animals, to contemplate the works of the Creator, and to look from Nature up to Nature's God."

In other respects, he was, in the language of another friend, "a careless man who never took care of anything," and indeed "he was a type of the *poco curante*," but "was liked by every one and regarded as very able in many departments of research."

II.

When Holbrook undertook the labor of monographing the reptiles of the United States, there was no one work to which reference could be made for information as to all the species. The nearest approach to it was a summary of the "Genera of North American Reptilia, and a Synopsis of the Species," by Richard Harlan, in the fifth and sixth volumes of the Journal of the Academy of Natural Sciences of Philadelphia (1826–'27). This itself furnishes excellent testimony to the desirability of a revision of the herpetology of the region in question. Most of the familiar forms of the Eastern and Middle States as well as South Carolina had indeed been described, but the descriptions and figures were in publications inaccessible to the ordinary reader. Many of the common reptiles had been early described by European zoölogists, and most of all by Linnæus, but a large proportion remained for incorporation in or rejection from the systems by native herpetologists. Say and Harlan had made known some; Green (1817 *et seq.*) had described the salamanders, and Leconte (1829) had monographed the turtles in a way; but much yet remained to be done in the examination of the many doubtful species and the incorporation of all the known forms in one comprehensive work. It was to this task that Holbrook applied himself.

Linnæus had entered about 30 species in the system; other European naturalists had named about 34 which are still recognized; previous American zoölogists had added nearly as many more (about 60) as the European. By the time that Holbrook had finished his work he had named 29 as new, and a very large proportion of these are still retained with his specific names, few having proved to be synonyms.

Holbrook must have outlined his work in herpetology, at least, soon after his settlement in Charleston and after he had entered upon the duties of his professorship. He engaged as an artist, for the representation of the reptiles, an Italian immigrant named J. Sera, as early as 1826, and retained him in his service till the man's death; but Holbrook gave no evidence of his activity till his work, in part at least, was ready for the press, not long before the artist's death, ten years later. Dr. Ogier has remarked that "this excellent artist had a particular fancy for drawing reptiles. We have often heard him say that he could never be satisfied with his work unless he gave 'the particular expression of his subject.' He was as enthusiastic about giving the 'peculiarly hard, cruel expression of the alligator's eye,' or 'the bright, deceitful look of the eye of the black snake,' as if his subjects belonged to the highest order of creation; and his drawings are indeed *fac similes* of the animals he intended to represent and monuments of his talent."

He had early, however, invoked the aid of various friends in different sections of the United States. His own endeavors in South Carolina were seconded by those of Dr. Ogier, Dr. Wurdeman, Dr. Baron, and Dr. Ravenel. In Massachusetts his own collecting was supplemented by that of Dr. Amos Binney and Dr. D. Humphreys Storer, of Boston. The reptiles of New York were collected by Messrs. Charles Hammond, Ogden Hammond, and Wilkens. Others that came to his aid were Dr. Geddings, of Baltimore; Dr. Harlan, of Philadelphia; Mr. T. L. Ogden, of Mobile, and Professor Troost, of Nashville. More general assistance was given by Major J. L. Leconte, then of New York, and, "above all," by Dr. Charles Pickering, of Philadelphia, who aided "with his accurate knowledge at every step of the work." Later Dr. Harden, of Georgia; Drs. Morton, Hallowell, and Blanding and Professor Green, of Philadelphia, and Dr. Dekay, of New York, gave aid. Scanty material from remote regions (Oregon and Texas) was furnished by Thomas Nuttall and A. Gaillard.

The self-imposed task grew upon him. Even in the preface to the first edition (p. v) he remarked:

"In undertaking the present work I was not fully aware of the many difficulties attending it; indeed, they could scarcely have been anticipated. With an immense mass of materials, without libraries to refer to, and only defective museums for comparison, I have constantly been in fear of describing animals as new that have long been known to European naturalists. In no department of American zoölogy is there so much confusion as in herpetology."

He long intended to supplement the generic definitions by fuller anatomical characteristics; for instance, in a foot-note to the first-described genus (*Testudo*) he indicated that it was his intention "in a subsequent number" to "add the special anatomy of each genus, illustrated by drawings" (I, 41), and in the last volume (III, 27) he referred to "the anatomical part of this work for a full description of the genus" *Kinosternon*. He also evidently intended to extend his publication in the anatomical supplement beyond anatomical features, and to develop some generalities, as in the preface to the second volume (p. 7) he promised that "in the anatomical part of this work it will be shown why one generic name is preferred to another." His intentions, however, were never realized.

Holbrook would have liked to have put his work in regular systematic form, but his determination to have his subjects described and painted from life, for the time being, prevented a strict adherence to such a desirable plan. He was in fact obliged to take the specimens as they came; consequently, the descriptions and plates were very much scattered in the first edition of his work. Evidently, too, other considerations than mere possession influenced him, for the common box tortoise, which certainly could have been easily obtained at any time, was not described till the third volume appeared. Many of the most common species, such as the alligator, the snapping turtles, the soft turtles, the glass snake, and numerous true snakes, were also left undescribed. He had apparently become dissatisfied with his own work and resolved to wait till he could procure as many of the species as possible, and then commence at the beginning again and issue a new edition in a systematic form.

For three years he had issued a volume nearly each year (1836–'38), but with the subjects irregularly presented, as will appear from the collation hereafter given. So particular was he as to proper coloration of the plates that, because in the first volume two were not colored from fresh specimens, he gave two others in substitution in an appendix to the second volume. Those were *Coluber erythrogrammus* (now *Abastor erythrogrammus*) and *Coluber abacurus* (now *Farancia abacura*). He further promised that whenever the coloring or attitude be faulty a duplicate will be added to the following volume.

The third volume being off his hands, Holbrook ceased publication for several years and devoted the time to the completion and systematization of his work. He accumulated all the materials he could, and put them all in as good systematic order as he was able. He adopted the classification then current, that which Duméril and Bibron had elaborated, and in this framework he introduced all the species inhabiting the United States, east and west, which he could obtain.

That classification, we now know, was a very artificial one, and by no means reflected nature, but for this Holbrook is not blamable. He was not a genius and had not access to large collections, and very naturally he followed the lead of those that had the use of what was then the greatest of all museums. He did the best he could under the circumstances, and that best was nearly, if not quite, equal to the best of what was done in Europe. The classification was based on very superficial characters, and it is possible that it was the contradiction which he observed between such features and the anatomical characters which his scalpel revealed that perplexed him and led to the abandonment of his intentions to give an anatomical supplement.

At last, in 1842, he had brought his work to such a form as was satisfactory enough to himself for publication, and he issued what was in fact an entirely new edition. This was the new " Herpetology of the United States," and was issued in complete form in five volumes. Much of the text had been modified and many of the plates of the first edition retouched, and the number increased by about 30 per cent. It was published with the following " publisher's note," signed " J. Dobson ":

" In consequence of the great number of new reptiles received by the author and the demand for the first three volumes, it became necessary either to reprint them or to make a new edition. The latter course has been preferred, thus enabling the author to introduce the new animals in their proper places and to add a number of new plates. It may be added that many of the plates have been reëngraved and improved."

The work thus completed embraced descriptions and illustrations of 147 nominal species, and few of them have proved to be other than real species in the present sense of the figure. Of these only 91 are now regarded a constituents of the restricted class of reptiles and 56 belong to the class of amphibians or batrachians. Comparatively few species have been added to the eastern fauna by subsequent gleaners in the old field, although among such are several quite common species—for instance, a tree-frog (*Hyla evittata*), abundant and readily found in special localities in and near the District of Columbia, was not discriminated till 1899, when Mr. Gerritt S. Miller, Jr., first described it. On the other hand, Holbrook made known several which have been overlooked till very recent times, as the *Salamandra quadrimaculata* (*Desmognathus quadrimaculata*), rediscovered by Stejneger in 1902.

Fortunately for the truth of his claims, he terminated his work shortly before the acquisition of Texas and California by the United States, and consequently it represented approximately the fauna of the country then possessed by the nation. Soon were to be added regions inhabited by a richer reptilian population, especially of the order of Saurians, and his successors would be able to more than double the number of species inhabiting the enlarged United States.

The descriptions are moderately good and full, but often evince a want of appreciation, or skill in contrast, of characters. He unfortunately did not follow in the footsteps of his French exemplars, Duméril and Bibron, in presenting the species of large genera in successively narrowed terms or dichotomously, and only gave, for ready comprehension, what he called "Characters," which he intended to be diagnostic, but which deficiency in skill sometimes prevented from being such. After the "Characters" followed the "Synonymes," then the "Description" (limited to external structural features), the "Colour," the "Dimensions," the "Habits," the "Geographical distribution," and, finally, "General remarks."

The illustrations were mostly fairly good, both for drawing and coloration, and quite equal to most of those published contemporaneously in Europe. The illustration was confined, however, to the bare animals, and no background (or ground to stand on) was ever represented, nor were any accessory figures illustrating details of structure furnished. Of the Tortoises, Saurians, and Batrachians, two figures were given on a single plate of each species, an upper or from a lateral aspect and a lower giving a view of the inferior surface, but both were more or less indirect or of the animal canted. Of the Snakes, a single figure suffices for each species; but the animal was twisted to give an idea of what might be seen from all points of view.

After the completion of the Herpetology, Dr. Holbrook and his wife paid a visit to Europe and Holbrook renewed acquaintance with some of the persons and scenes he knew in his youth. At the Muséum d'Histoire Naturelle of Paris, according to Dr. Ogier, he "was received with open arms by Valenciennes and other naturalists in 'the Jardin des Plantes,'" and was invited "by those in charge of the museum" to identify or confirm the accuracy of previous identification of North American reptiles. Holbrook often spoke "of this as one of the greatest compliments paid to his knowledge of reptiles."

III.

The work on herpetology having been completed, and Holbrook having again become settled at home, he now devoted his energy to the preparation of a companion work on the fishes.

Systematic ichthyology, as generally understood, was then in the condition it was placed in 1829 by Cuvier in the second edition of the Règne Animal. A comparatively few large families were recognized, and the series was headed by the Perches.

Although the production of the first comparative anatomist of the age, it was distinguished by the prominence given to superficial characters and the neglect of deeper-seated ones, and especially of osteological peculiarities. The last volume—twenty-second—of the great Histoire Naturelle des Poissons, begun by Cuvier and Valenciennes and continued by the last, was published in 1849, and Storer, in 1846, had compiled, chiefly from it, a poor "Synopsis of the Fishes of North America." Dekay, a few years previously (1842), had published the finely illustrated part on Fishes of his "New York Fauna," also adopting the Cuvierian system.

From the same point of view Holbrook commenced his work. He recognized that he could not cover so large a field as he had done for the reptiles, and instead of all the United States, he would limit his attention to the fishes of the "Southern States." With Richard as his artist, he brought out, in 1847, "number two" of a "Southern Ichthyology; or a Description of the Fishes Inhabiting the Waters of South Carolina, Georgia, and Florida." This bore the imprint of "New York and London: Wiley & Putnam, 1847." It contains 32 pages (1–32) and 4 plates, illustrating *Umbrina alburnus* (1, I, 1), *U. littoralis* (10, I, 2), *Micropogon undulatus* (12, II, 2), *Corvina ocellata* (17, II, 1), *Leiostomus obliquus* (21, III, 1), *Lobotes surinamensis* (25, III, 2), *Elacate canada* (30, IV, 1), and *Ephippus gigas* (IV, 2). It is announced on the cover of this second part that "No. 1, containing the anatomical portion of the work, will be published with No. VI," and in the "Notice" to the first edition of the work reviewed, it is affirmed that "two numbers were published under another title in 1845." The number noticed is, however, the only one with which we are acquainted.

The statement is made in the form of a "Notice," dated "November 10th, 1854," issued with the first number of the "Ichthyology of South Carolina," that "much of this work now offered to the public was printed several years since; indeed, two numbers were published under another title in 1845; some few pages have been reprinted and new matter added. So much it is necessary to say to account for the apparent negligence in not referring to late works on ichthyology."

It may be reasonably suspected that the "*poco curante*" habit of the author is responsible for a slight mistake here. Diligent inquiry has failed to discover any other evidence of printing of any number but "number two." That number is recorded in "A Dictionary of Books Relating to America," by Joseph Sabin (VIII, 368), and no other. In the lapse of time the author may have assumed that, inasmuch as "number two" had been published, number one must also have been printed. and, trusting to an imperfect memory, that 1845 was the date rather than 1847. At any rate, until more evidence is furnished, we are almost. perforce, compelled to believe that "number two" was the only one of the "Southern Ichthyology" published.

When this part had been issued Holbrook must have made a survey of the field he wanted to cover; he must have formed some estimate of the number of West Indian fishes which could be found along the southern Floridian coast and realized how impossible it would be, under the circumstances, to realize his desire for describing and painting his subjects only from life. He paused and paused, finally gave up his intention to wander over so large a field, and at length determined to confine his efforts to the fishes of his own state.

At last, in 1855, he commenced the publication of the "Ichthyology of South Carolina" in parts, and ten of these were issued when the further publication was interrupted by a fire which destroyed the "Artists' Buildings" in Philadelphia, where the pictorial portion of the work was being prosecuted. The original drawings, as well as plates and stones, were all destroyed.

Holbrook took advantage of this loss to better his work in various ways. He explained the circumstances which led to the new edition in his "preface," here reproduced in part:

"The great delay in the publication of the Ichthyology of South Carolina has been caused by the destruction of all the plates, stones, and original drawings in the burning of the 'Artists' Buildings,' in Philadelphia, several years since.

"This made it necessary to have new drawings made of all the different fishes, which has been done at great expense, so great, indeed, that the work could not have been carried on without the aid of the State, which has been freely given.* The new

*I have been unable to find any act of appropriation passed by the legislature of South Carolina for aid to Holbrook's work. A work of kindred character ("Tuomey and Holmes' Fossils of South Carolina") drawings are from nature, and have been made by the best artists, as A. J. Ibbotson and A. Sonrel. The color of the fish has been, in almost every instance, taken from living specimens by J. Burkhardt, an artist of great merit.

"The delay in the publication of the work has, however, enabled me to give more accurate and highly finished plates, and to correct some errors in the letter press.

"As but few numbers of the work were distributed previous to the destruction of the original plates, &c., and the present edition is so much improved, I have decided to recall the former numbers and to replace them by those of the new edition."

Thus an entirely new work was published. The artistic efforts of Richard were superseded by the superior results of Ibbotson and Sonrel, both excellent artists trained under Agassiz, and T. Sinclair's lithographic establishment of Philadelphia was selected for reproducing the illustrations instead of Tappan & Bradford's of Boston; the printing was done by Welch, Bigelow and Company, of Cambridge, in place of Metcalf and Company, and the publishers were "Russell & Jones," in succession to "John Russell," of Charleston, S. C.

Comments on both editions made by the present writer in the American Journal of Science and Arts for 1864, soon after the actual publication of the second edition, are as applicable now as then, and are consequently reproduced.

"In the second edition, the generic and specific descriptions are in most cases entirely the same as those of the first, the principal deviations occurring in the family called Ichthelidæ, which is newly named and defined. The plates are also arranged in the same manner, the only exception relating to xxiii and xxiv, which had the numbers reversed in the first, and the interposition of an additional plate between xxvi and xxvii, which latter, in the second edition, is consequently called xxviii. The figures themselves are mostly new and are, as a rule, superior to those of the original edition; the worst of the first edition are those illustrating the scales of the Sparoid fishes, and another intended to represent the preoperculum of '*Homo-*

was appropriated for and supported six years. To "the sixth year's subscription" of $2,000, a proviso was attached "that no further subscription be made for the said work." (Acts South Carolina, November, 1860, to January, 1861, Statutes at Large, xii, p. 847.)

prion lanceolatus.' Dr. Holbrook, adopting the fashion introduced into this country, of figuring three scales of each species, has caused to be thus represented those of the Sparoids, but none in the first edition give an idea of the type of structure peculiar to the representatives of that family and so characteristic of it. When the scales are so especially figured, we might at least reasonably expect a close approximation to correctness, and when it is not found, and it thus becomes apparent that the author himself has not paid special regard to them, we may well ask why the time and space given to these figures could not have been more advantageously bestowed in illustrating more important characters. By what strange optical delusion a preoperculum, like that represented in the enlarged view of that bone in *Homoprion lanceolatus*, could have been imagined by the artist, is difficult to conjecture. With these remarks, however, special criticism may end, for although some of the other figures might be much improved, most are accurate and compare favorably with the best of those published elsewhere."

Under *Homoprion xanthurus* the *specific character* is based on an extract from Cuvier and Valenciennes' description and radial formula of *Leiostomus xanthurus*, while the body of the description and the figure apply to *Bairdiella argyroleuca*, the *Corvina argyroleuca* C. and V., a species of a very different subfamily. If Dr. Holbrook had been correct in his application of Lacépède's name *Leiostomus xanthurus*, he would have been subject to a charge of a perversion of that author's generic name, but by a happy error he has correctly retained it in its true sense.

"On the other hand, some former names, concerning whose application there is no reasonable room for doubt, have not been at all accepted, such as the Linnæan *Labrus auritus* and *Gasterosteus carolinus*. The former was evidently proposed for the species called by Holbrook *Ichthelis rubricauda*, the *Pomotis rubricauda* of Storer, well characterized in the terse Linnæan phrase 'opercula apice membranaceo, *elongato*, obtuso, *nigro*,' and even rendered more certain as to its application by the *doubtful* reference to Catesby's figure of *Pomotis aureus*. It is, however, due to Dr. Holbrook to state that it appeared to him 'certain that the specific name *auritus* was not applied to the *Pomotis vulgaris*,' and that Linnæus's description might 'possibly apply to' either *P. rubricauda* or *P. incisor*. Probably none familiar with the subject will hesitate to retain the *Linnæan* name instead of *rubricauda*. The *Gasterosteus carolinus* was as evidently intended for Holbrook's *Bothrolæmus pompanus*, notwithstanding this author's opinion to the contrary. The latter species, it may be here remarked, has served, at different stages of development, as the type of three genera, and Holbrook's *Bothrolæmus* is founded simply on very old individuals of *Trachynotus* in which the teeth had fallen out.

"As Dr. Holbrook has not uniformly adopted a systematic arrangement, but has scattered some species in places where they do not belong, the species given under a family name cannot be considered as members of that family, even in the author's opinion, and many of those have been referred to their proper ones in foot-notes to the text. *Labrax*, *Grystes*, *Serranus*, *Diplectrum*, *Rhypticus*, and *Centropristes* are not Ichthelidæ, but Percidæ; * *Pagrus* and *Serranus nigritus* not Sciænidæ, but severally Sparoid and Percoid; and finally *Trachinotus* and *Hæmulon* are not 'Scopelinidæ,' but respectively members of the Scombroid and Sciænoid families as understood by Dr. Holbrook.

"With regard to the systematic arrangement thus corrected, it may be remarked that it is not an exposition of the views now prevalent concerning the limits of the families. All the Scombridæ of Holbrook are Carangoids, except *Cybium*, *Elacate*, *Echeneis*, and *Temnodon*, members of as many different families. *Ephippus* scarcely belongs to the same family as *Chætodon* and its allies; *Hæmulon* and *Pristopoma* are nearer Sparoids than Sciænoids, and at least do not belong to the latter family.† *Lobotes* is the type of a peculiar one, and finally *Saurus* is the representative of another.

"The most important modification in the arrangement is undoubtedly the foundation of the family Ichthelidæ for the reception of the North American fresh-water Percoids of Cuvier with six branchiostegal rays. Adopting the family of Percidæ with

* *Labrax*, *Serranus*, *Diplectrum*, *Rhypticus*, and *Centropristes* were later segregated from the Percidæ by the writer into the nearly related family of Serranidæ and *Grystes* (under the name *Micropterus*) was referred to the family *Centrarchidæ*.

† *Hæmulon* and *Pristopoma* were later referred to the family *Hæmulidæ*, which is much nearer the Sparoids than the Sciænoids.

the boundaries established for it by Sir John Richardson, he has considered that the Theraponidæ of that author taken from it should be itself subdivided, and the family of Ichthelidæ is therefore proposed for some of its constituents. The only positive character of the family mentioned by Holbrook which would remove it from the typical Percoids is the presence of only six branchiostegal rays. As such, if strictly adhered to, would necessitate the expulsion from the latter of *Dules* (*auriga*), *Percilia*, etc., and their transference to the Ichthelidæ, the character is not the true one, and is of very secondary importance in itself. The group of genera embraced under Ichthelidæ is, however, so natural and its representatives so well distinguished from the true Percoids by their physiognomy that it is probable that the family itself is a natural one. It has indeed more resemblance to the Cichloids, and its species hold the same place in North America that those fishes do in the southern continent and in Africa. Like them, the Ichthelidæ construct a rude nest, guard their young, and are the most characteristic Acanthopterygian types of their respective regions. Their arrangement of colors and the variation in the number of anal spines are analogous and their forms simulate each other. That form is distinguished by the equal development of and the correspondence of the regions of the body above and below the axis, while in the Percoids and others those regions are obliquely opposed."*

The descriptions of the fishes were made on the same general plan as those of the reptiles, and the remarks made on the latter are applicable to the former. First was given a quasi-diagnosis after the caption "Specific characters;" then the "Synonymes;" next followed the "Description," the "Colour," the "Dimensions," the "Splanchnology," the "Habits," the "Geographical distribution," and, lastly, "General remarks" respecting relationship, nomenclature, or history. The data respecting splanchnology or abdominal viscera are in small type (brevier), while the rest is in long primer.

The illustrations represent the fish from a direct side view, with the fins extended as much as possible, and there are mag-

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* The family *Ichthelidæ* was later named *Centrarchidæ* by the writer and is now generally adopted with the latter name and established on osteological characters. The analogical resemblance of the Centrarchoids and Cichloids was first recognized in the article of 1864 quoted.

nified views of three scales of almost every species, one from the lateral line, one from the back, and one from the belly. As already indicated, the supervision over the artist and correction of his work must have been rather lax.

IV.

Soon after the printing of this work, the civil war, in which South Carolina took so prominent a part, began. Like all other eminent men, Holbrook was obliged to become a participant, and his medical knowledge was utilized by his selection as the "head of the examining board of surgeons in South Carolina."

In 1863 his wife died, and he was left childless and alone. When the forces of the Union took possession of Charleston, the medical college in which his collections were preserved was taken for hospital purposes, and his specimens were wantonly thrown away or seized upon for what they were supposed to be worth; his books were stolen, and finally his drawings and manuscripts were lost or destroyed. An old man now, bereft of most of his fortune, discouraged by adversities, and recognizing that a new order of scientific procedure had begun, he reluctantly ceased to even plan for his work. He continued, however, to go in summer to New England, where he had spent his happy youth, and in the early fall of 1871 (8th of September), stricken by apoplexy, saw the end of life, at his sister's residence in Norfolk, Massachusetts, "breathing his last amidst kind and devoted relatives."

Holbrook was elected a member of the National Academy of Sciences in 1868, during its January session. Had he been in the North at the time of the formation of the Academy (1863), he would probably have been one of its founders; but then he was widely separated by distance as well as by war and sympathies.

The memory of Holbrook has been recalled, in the manner customary to naturalists, in connection with various reptiles, amphibians, and fishes, by a number of zoölogists of eminence. In his honor were named the typical species of *Scaphiopus* by Harlan (1840), one of *Tropidonotus* by Baird and Girard (1853), one of *Callopeltis* by Duméril and Bibron (1854), one of *Trachemys* by Gray (1857), and one of *Lampropeltis* by Stejneger (1902).

Ichthyologists also honored him in such names as *Pomotis* *holbrookii* (Cuvier and Valenciennes, 1831), *Alutera holbrookii* (Hollard, 1855), *Heterandria holbrookii* (Agassiz, 1859), *Echeneis holbrookii* (Günther, 1860), *Acipenser holbrookii* (Duméril, 1867), *Ophidium holbrookii* (Putnam, 1874), and *Diplodus holbrookii* (Bean, 1878). It is proper to add, however, that most of these names have been shown to be synonyms of older ones.

Those who believe in the influence of heredity and association in the determination of tastes or avocations may have their faith fortified by the knowledge that a younger and the only full brother of the naturalist (Silas Pinckney Holbrook, born 1796), although educated for the law, devoted himself to literary pursuits and in his comparatively short life (he died in 1835) contributed much to the periodical and other literature of the country. Although his literary productions were almost entirely published in the North, he went to the South in 1835 and died at Pineville, South Carolina, May 26.

The author is indebted to Dr. Marcus Benjamin for the loan of the originals from which the portrait and letter of Holbrook have been reproduced. These, as well as various other data which were lent, were obtained by Dr. Benjamin in 1887, when he was preparing the biographical notices of members of the National Academy of Sciences for Appletons' Cyclopædia of American Biography.

Both letter and portrait are undated, but the former was evidently written just before his departure for Europe with his wife, about 1842, and the latter was probably taken about the same time, the copy owned by Dr. Benjamin having apparently been reproduced from an old-fashioned daguerreotype. (The daguerreotype was introduced no earlier than 1839.) The neckwear or "stock" also points to that time, and the appearance is that of a man certainly not more than 48 years old, which age Holbrook had attained in 1842.

Bibliography.

The bibliography of Holbrook is by no means extensive. Indeed, there are very few eminent naturalists who have published so few articles. Except the large works on reptiles and fishes, only one contribution is known, an article published in the *Journal* of the Academy of Natural Sciences of Philadelphia. No article on any subject of medical or surgical practice appears in any southern or other medical periodical.

The difficulties incident to the consultation of his works may be inferred from the fact that only one edition of each of his great works is possessed by the wealthiest and most progressive zoölogical society in the world. From the fifth edition of the "Catalogue of the Zoölogical Society of London" (1902), it appears that only four volumes of the North American Herpetology are in its library (the fifth being "wanting") and only the incomplete first edition of the "Ichthyology of South Carolina" is there. The Academy of Natural Sciences, whose library is, perhaps, the best zoölogical one in the United States, is also deficient.

Further, most of the notices of Holbrook, and even the very extended "Dictionary" of Sabin, are replete with errors in mention or description of his works.

On account of this rarity or inaccessibility of all editions of his works, Dr. Stejneger and the writer have correlated the two, and the results are here presented with the addition of the family names adopted by Holbrook. In the first column, the names of Holbrook, and in the last those of the "modern nomenclature" are given. The sequence is that of the last edition, as is also the nomenclature, but when different the nomenclature of the first edition is indicated by indented names.

In order to collate the different editions of the "Ichthyology of South Carolina," the writer was obliged to use three libraries, those of the Smithsonian Institution, the Academy of Natural Sciences of Philadelphia, and the Brevoort, now owned by the American Museum of Natural History.

The collation and description are rendered difficult and perhaps at first incomprehensible by the method of pagination characteristic of old Philadelphia printers (but not of B. Franklin!). They were in the habit of beginning the pagination

CORRELATIONS OF FIRST AND SECOND EDITIONS—Continued.

Holbrook's nomenclature.	First edition. Vol.	Page.	Plate.	Second ed. Vol. II. Page.	Plate.	Modern nomenclature.
ORDER II. SAURIA. *Brogniart.*						
FAMILY. CROCODILIDA. *Cuvier.*						
Alligator Mississippiensis				53	vii	*Alligator mississippiensis.*
FAMILY. IGUANIDA. *Dumeril et Bibron.*						
Anolius Carolinensis	I,	67	vii	67	viii	*Anolis carolinensis.*
Tropidolepis undulatus	III,	51	viii	73	ix	*Sceloporus undulatus.*
Crotaphytus collaris (n.)				79	x	*Crotaphytus collaris.*
Phrynosoma cornuta	III,	55	ix	87	xi	*Phrynosoma cornutum.*
orbiculare		61	x	93	xii	*Phrynosoma cornutum.*
coronata		65	xi	97	xiii	*Phrynosoma blainvillei.*
douglassii		69	xii	101	xiv	*Phrynosoma douglassii.*
FAMILY. LACERTINIDA. *Dumeril et Bibron.*						
Ameiva sexlineata	I,	63	vi	109	xv	*Cnemidophorus sexlineatus.*
FAMILY. SCINCOIDEA.						
Plestiodon erythrocephalus				117	xvi	*Eumeces fasciatus.*
(*Scincus erythrocephalus*)	II,	101	xxii	....		
Scincus quinquelineatus	III,	39	vi	121	xvii	*Eumeces fasciatus.*
fasciatus		45	vii	127	xviii	*Eumeces fasciatus.*
Lygosoma lateralis				133	xix	*Leiolopisma laterale.*
(*Scincus lateralis*)	I,	71	viii	.....	....	
FAMILY. CHALCIDA.						
Ophisaurus ventralis				139	xx	*Ophisaurus ventralis.*
ORDER III. OPHIDIA. *Brogniart.*						
FAMILY. CROTALOIDEA.				Vol. III.		
Crotalus durissus	II,	81	xvii	9	pl. i	*Crotalus horridus.*
adamanteus		77	xvi	17	ii	*Crotalus adamanteus.*
oregonus (n.)				21	iii	*Crotalus oregonus.*
Crotalophorus miliarius				25	iv	*Sistrurus miliarius.*
(*Crotalus miliarius*)	II,	73	xv			
Crotalophorus tergeminus				29	v	*Sistrurus catenatus.*
kirtlandi (n.)				31	vi	*Sistrurus catenatus.*
Trigonocephalus piscivorus	II,	63	xiii	33	vii	*Agkistrodon piscivorus.*
contortrix		69	xiv	39	viii	*Agkistrodon contortrix.*
atro-fuscus				43	ix	?
FAMILY. ELAPSOIDEA.						
Elaps fulvius	II,	87	xviii	49	x	*Elaps fulvius.*
FAMILY. COLUBEROIDEA.						
Coluber constrictor				55	xi	*Bascanion constrictor.*
obsoletus				61	xii	*Callopeltis obsoletus.*
testaceus				63	xiii	*Bascanion testaceum.*

CORRELATIONS OF FIRST AND SECOND EDITIONS—Continued.

Holbrook's nomenclature.	First edition. Vol.	Page.	Plate.	Second ed. Page.	Plate.	Modern nomenclature.
				Vol. III.		
Coluber guttatus	II	109	xxiv	65	xiv	*Callopeltis guttatus.*
eximius				69	xv	*Lampropeltis triangula.*
couperi (n.)				75	xvi	*Drymarchon couperi.*
vernalis				79	xvii	*Liopeltis vernalis.*
punctatus	II	115	xxvi	81	xviii	*Diadophis punctatus.*
alleghaniensis	I	111	xx	85	xix	*Callopeltis obsoletus.*
quadrivittatus (n.)		113	xxi	89	xx	*Callopeltis quadrivittatus.*
Coronella getula				95	xxi	*Lampropeltis getulus.*
sayi				99	xxii	*Lampropeltis holbrooki.*
rhombo-maculata (n.)				103	xxiii	*Lampropeltis rhombomaculata.*
doliata				105	xxiv	*Lampropeltis doliatus.*
Helicops erythrogrammus				107	xxv	*Abastor erythrogrammus.*
(*Coluber erythrogrammus*)	I	115	xxii			
Helicops abacurus (n.)				111	xxvi	*Farancia abacura.*
(*Coluber abacurus*)	I	119	xxiii			
Brachyorrhos amœnus				115	xxvii	*Carphophis amœnus.*
Calamaria elapsoidea (n.)				119	xxviii	*Lampropeltis elapsoides.*
(*Coluber elapsoides*)	II	123	xxviii			
Calamaria striatula				123	xxix	*Haldea striatula.*
Rhinostoma coccinea				125	xxx	*Cemophora coccinea.*
				Vol. IV.		
Pituophis melanoleucus				7	pl. i	*Pituophis melanoleucus.*
Psammophis flagelliformis				11	ii	*Bascanion flagellum.*
(*Coluber flagelliformis*)	I	107	xix			
Leptophis æstivus				17	iii	*Opheodrys æstivus.*
(*Coluber æstivus*)	II	119	xxvii			
Leptophis sauritus				21	iv	*Thamnophis sauritus.*
Tropidonotus fasciatus				25	v	*Natrix fasciata.*
(*Coluber fasciatus*)	II	93	xx			
Tropidonotus sipedon				29	vi	*Natrix sipedon.*
erythrogaster				33	vii	*Natrix erythrogaster.*
(*Coluber erythrogaster*)	II	91	xix			
Tropidonotus taxispilotus (n)				35	viii	*Natrix taxispilotus.*
(*Coluber taxispilotus*)	II	113	xxv			
Tropidonotus niger				37	ix	*Natrix sipedon.*
rigidus				39	x	*Natrix rigida.*
sirtalis				41	xi	*Thamnophis sirtalis.*
ordinatus				45	xii	*Thamnophis ordinatus.*
leberis				49	xiii	*Natrix leberis.*
dekayi (n.)				53	xiv	*Storeria dekayi.*
Heterodon simus				57	xv	*Heterodon simus.*
niger	II	105	xxiii	63	xvi	*Heterodon platirhinos.*
platirhinos		97	xxi	67	xvii	*Heterodon platirhinos.*

ORDER IV. BATRACHIA. *Brogniart.*

SUBORDER II. ECAUDATA. *Oppel. Dumeril et Bibron.*

FAMILY I. RANOIIDEA.

Rana pipiens	III	81	xv	77	xviii	*Rana catesbeiana.*
horiconensis		91	xviii	83	xix	*Rana clamitans.*
clamitans		89	xvii	85	xx	*Rana clamitans.*

CORRELATIONS OF FIRST AND SECOND EDITIONS—Continued.

Holbrook's nomenclature.	First edition. Vol.	Page.	Plate.	Second edition. Vol. IV. Page.	Plate.	Modern nomenclature.
Rana fontinalis		85	xvi	87	xxi	*Rana clamitans.*
halecina	I,	89	xiii	91	xxii	*Rana pipiens.*
palustris		93	xiv	95	xxiii	*Rana palustris.*
sylvatica		95	xv	99	xxiv	*Rana sylvatica.*
Cystignathus ornatus (n.)				103	xxv	*Chorophilus ornatus.*
(*Rana ornata*)	I,	97	xvi			
Cystignathus nigritus (n.)				107	xxvi	*Chorophilus nigritus.*
(*Rana nigrita*)	III,	93	xix			
Scaphiopus solitarius (n.)	I,	85	xii	109	xxvii	*Scaphiopus holbrookii.*
FAMILY II. HYLOIDEA.						
Hyla versicolor	I,	101	xvii	115	xxviii	*Hyla versicolor.*
viridis	III,	95	xx	119	xxix	*Hyla cinerea.*
squirella	I,	105	xviii	123	xxx	*Hyla squirella.*
femoralis				127	xxxi	*Hyla femoralis.*
delitescens				129	xxxii	?
Hylodes gryllus	III,	75	xiii	131	xxxiii	*Acris gryllus.*
pickeringii (n.)				135	xxxiv	*Hyla pickeringii.*
ocularis	III,	79	xiv	137	xxxv	*Chorophilus ocularis.*
FAMILY III. BUFONOIDEA.						
				Vol. V.		
Bufo lentiginosus				7	pl. i	*Bufo lentiginosus.*
(*Bufo clamosus*)	I,	79	xi			
erythronotus	III,	99	xx	11	ii	?
quercicus (n.)				13	iii	*Bufo quercicus.*
americanus	I,	75	ix	17	iv	*Bufo americanus.*
cognatus				21	v	*Bufo cognatus.*
Engystoma carolinense (n.)	I,	83	x	23	vi	*Engystoma carolinense.*
FAMILY II. CAUDATA.						
Salamandra gutto-lineata	II,	61	xii	29	vii	*Spelerpes guttolineatus.*
salmonea	III,	101	xxii	33	viii	*Spelerpes porphyriticus.*
rubra				35	ix	*Spelerpes ruber.*
glutinosa				39	x	*Plethodon glutinosus.*
erythronota	III,	113	xxvii	43	xi	*Plethodon cinereus.*
auriculata (n)		115	xxviii	47	xii	*Desmognathus auriculata.*
quadrimaculata (n)				49	xiii	*Desmognathus quadrimaculata.*
jeffersoniana				51	xiv	*Ambystoma jeffersonianum.*
cirrigera				53	xv	*Spelerpes bislineatus.*
bilineata				55	xvi	*Spelerpes bislineatus.*
symmetrica	II,	59	xi	57	xvii	*Diemictylus viridescens.*
haldemani (n)				59	xviii	?
longicauda	III,	111	xxvi	61	xix	*Spelerpes longicaudus.*
granulata				63	xx	*Ambystoma jeffersonianum.*
quadridigitata (n.)				65	xxi	*Manculusquadridigitatus.*

CORRELATIONS OF FIRST AND SECOND EDITIONS—Continued.

Holbrook's nomenclature.	First edition. Vol.	Page.	Plate.	Second edition. Nol. V. Page.	Plate.	Modern nomenclature.
Salamandra venenosa	III,	105	xxiv	67	xxii	*Ambystoma maculatum.*
fasciata		103	xxiii	71	xxiii	*Ambystoma opacum.*
talpoidea (n.)		117	xxix	73	xxiv	*Ambystoma talpoideum.*
Triton dorsalis				77	xxv	*Diemictylus viridescens.*
(*Salamandra dorsalis*)	II,	57	x			
Triton tigrinus				79	xxvi	*Ambystoma tigrinum.*
(*Salamandra tigrina*)	III,	109	xxv			
Triton niger				81	xxvii	*Desmognathus nigra.*
porphyriticus				83	xxviii	*Spelerpes porphyriticus.*
ingens				85	xxix	*Ambystoma tigrinum.*
TRIBE II. IMMUTABILIA.						
FAMILY I. CRYPTOBRANCHOIDEA.						
Amphiuma means				89	xxx	*Amphiuma means.*
tridactylum				93	xxxi	*Amphiuma means.*
Menopoma alleghaniensis				95	xxxii	*Cryptobranchus alleghaniensis.*
fusca (n.)				99	xxxiii	*Cryptobranchus fuscus.*
FAMILY II. PHANEROBRANCHOIDEA.						
Siren lacertina				101	xxxiv	*Siren lacertina.*
intermedia				107	xxxv	*Siren lacertina.*
striata				109	xxxvi	*Pseudobranchus striatus.*
Menobranchus maculatus				111	xxxvii	*Necturus maculosus.*
lateralis	III,	119	xxx	115	xxxviii	*Necturus maculosus.*

III.

Southern Ichthyology: or, a Description of the Fishes inhabiting the Waters of South Carolina, Georgia, and Florida. No. 2. New York: Wiley & Putnam. 1847. [4to, pp. 1–32, colored plates i–iv.]

No more appears to have been published; see page 59.

The species described and illustrated and the names now generally accepted are herewith given:

Holbrook's names.	Page.	Plate.	Fig.	
Umbrina alburnus	1	I	1	*Menticirrus alburnus.*
Umbrina littoralis	10		2	*littoralis.*
Micropogon undulatus	12	II	2	*Micropogon undulatus.*
Corvina ocellata	17		1	*Sciænops ocellatus.*
Leiostomus obliquus	21	III	1	*Leiostomus xanthurus.*
Lobotes surinamensis	25		2	*Lobotes surinamensis.*
Elacate canada	30	IV	1	*Elacate canada.*
Ephippus gigas		IV	2	*Chætodipterus faber.*

IV.

An account of several species of Fish observed in Florida, Georgia, &c. *Journ. Acad. Nat. Sci. Phila.*, III, Art. V, pp. 47–58, pls. v, vi, 1855.

Descriptions and illustrations of eight fresh-water fishes are published, viz:

Holbrook's names.	Page.	Plate.	Fig.	Modern names.
Pomotis elongatus	47	v	1	*Lepomis auritus solis.*
speciosus	48	"	2	*holbrooki.*
marginatus	49	vi	2	*megalops.*
Bryttus fasciatus	51	v	3	*Enneacanthus obesus.*
gloriosus	52	"	4	*Enneacanthus gloriosus.*
Calliurus floridensis	53	vi	1	*Chænobryttus gulosus.*
Pimelodus marmoratus	54	"	4	*Ameiurus nebulosus marmoratus.*
Boleosoma Barratti	56	"	3	*Boleichthys fusiformis.*

V.

Ichthyology | of | South Carolina. | By John Edwards Holbrook, M. D., | [7 lines of titles] | Charleston, S. C.: | Published by John Russell. | 1855 [-1857]. [4to, t. p. (= 1 l.) + notice (= 1 l.) + pp. 1–184, pl. 1–29 + pl. unnumbered.] 27 colored plates.

Only the date 1855 is given on the title page, but the issue of the parts extended over nearly three years. The author's "notice" is dated "November 10th, 1854."

VI.

Ichthyology | of | South Carolina. | By John Edwards Holbrook, M. D., | [7 lines of titles.]—Vol. I. | Charleston, S. C.: | Published by Russell and Jones. | 1860. | [4to, vii + 205 pp., 28 pl.]

The species described and illustrated are enumerated in the following correlation of both editions:

CORRELATION OF EDITIONS OF 1855–'57 AND 1860 OF HOLBROOK'S FISHES OF SOUTH CAROLINA.

Holbrook's names.	Text. 1. ed.	Text. 2. ed.	Illustrations. Pls.		Modern names.
FAMILY PERCIDÆ.					
Perca flavescens	2	2	i,	1	*Perca flavescens.*
FAMILY ICHTHELIDÆ.					
Pomotis vulgaris	6	8	i,	2	*Eupomotis gibbosus.*
Ichthelis incisor		12	ii,	1	*Lepomis pallidus.*
(*Pomotis incisor*)	13			1	

Correlations—Continued.

Holbrook's names.	Text. 1. ed.	Text. 2. ed.	Illustrations.	Pls.	Modern names.
Ichthelis rubricauda..........		15		2	*Lepomis auritus*.
(*Pomotis rubricauda*).....	10			2	
Centrarchus irideus...........		18	iii,	1	*Centrarchus macropterus*.
	15			1	*Enneacanthus obesus*.
Labrax Americanus...........		20		2	*Morone americana*.
(*Labrax rufus*).......	21			2	
Labrax lineatus..............	17	24	iv,	1	*Roccus lineatus*.
Grystes Salmoides.............	25	28		2	*Micropterus salmoides*.
Serranus erythrogaster.	29	32	v,	2	*Epinephelus morio*.
Diplectrum fasciculare.........	32	35		1	*Diplectrum fasciculare*.
Pomoxis hexacanthus.....		39	vi,	1	*Pomoxis hexacanthus*.
(*Centrarchus hexacanthus*).	36			1	
Rhypticus maculatus (n.)		42		2	*Rypticus maculatus*.
(*Rypticus maculatus*)......	39			2	
Centropristes atrarius..........	42	45	vii,	2	*Centropristes striatus*.
trifurca.........	47	49		1	*Centropristes philadelphicus*
Family Sparidæ.					
Sargus ovis..................	51	53	viii,	2	*Archosargus probatocephalus*.
Lagodon rhomboides		59		1	
(*Sargus rhomboides*)	56			1	
Family Scombridæ.					
Temnodon saltator.....	62	64	ix,	2	*Pomatomus saltatrix*.
Cybium maculatum...........	66	68		1	*Scomberomorus maculatus*.
Seriola carolinensis (n.).......	70	72	x,	2	*Seriola carolinensis*.
zonata...	73	75		1	*Seriola zonata*.
chloris................		79	xi,	1	*Chloroscombrus chrysurus*.
(*Seriola cosmopolita*)......	77			1	
Bothrolæmus pampanus (n.) ..	81	83		2	*Trachynotus carolinus*.
Caranx defensor..........	85	87	xii,	1	*Carangus chrysos*.
hippos...............	88	90		2	*Carangus hippos*.
falcatus (n.)........ ..	92	94	xiii,	2 (sup.)	*Hemicaranx amblyrhynchus*.
Richardi (n.).........	94	96	xiii,	1 (inf.)	*Carangichthys latus*.
Elacate canada..............		97	xiv,	2	*Elacate canada*.
	95			1	
Echeneis lineata (n.)..........		102	xiv,	1	*Echeneis neucrates*.
(*Echeneis albicauda*)......	101			2	
Family Squamipinnidæ.					
Ephippus gigas...............	105	107	xv,	2	*Chætodipterus faber*, old.
faber..........	108	110		1	*Chætodipterus faber*, young.

CORRELATIONS—Continued.

Holbrook's names.	Text. 1. ed.	2. ed.	Illustrations.	Pls.	Modern names.
FAMILY SCIÆNIDÆ.					
Pogonias cromis	112	114	xvi,	2	*Pogonias chromis*, old.
fasciatus	118	119		1	*Pogonias chromis*, young.
Hæmulon chrysopteron	120	121	xvii,	1	*Hæmulon chrysopteron.*
arcuatum	123	124		2	*Hæmulon plumieri.*
Otolithus regalis	127	129	xviii,	1	*Cynoscion regalis.*
thalassinus (n.)	132	133		2	*Cynoscion thalassinus.*
nothus (n.)	134	134	xix,	1	*Cynoscion nothus.*
carolinensis	133	136		2	*Cynoscion nebulosus.*
Umbrina alburnus	136	137	xx,	1	*Menticirrus alburnus.*
littoralis (n.)	142	144		2	*Menticirrus littoralis.*
Micropogon undulatus	145	146	xxi,	1	*Micropogon undulatus.*
Corvina ocellata	149	150		2	*Sciænops ocellatus.*
Larimus fasciatus (n.)	153	154	xxii,	1	*Larimus fasciatus.*
Pristipoma fulvomaculatum		157		2	*Orthopristis fulvomaculatus.*
(*Hæmulon fulvomaculatum*).	156			2	
Leiostomus obliquus		160	xxiii,	1	*Leiostomus xanthurus.*
	163		xxiv,	1	
Homoprion xanthurus		164	xxiv,	2	*Bairdiella chrysura.*
	170		xxiv,	2	
lanceolatus (n.)		167	xxiv,	1	*Stellifer lanceolatus.*
	168		xxiii,	1	
Lobotes surinamensis	159	169		2	*Lobotes surinamensis.*
Pagrus argyrops	175	174	xxv,	1	*Stenotomus chrysops.*
Serranus nigritus (n.)	173	177		2	*Garrupa nigrita.*
FAMILY ELOPIDÆ.					
Elops saurus	179	180	xxvi,	2	*Elops saurus.*
FAMILY SCOPELINIDÆ.					
Saurus fœtens	184	187	xxvi,	1	*Synodus fœtens.*
Trachinotus glaucus		192	xxviii,	1	*Trachynotus glaucus.*
Hæmulon quadrilineatum		185		2	*Bathystoma rimator.*
FAMILY ESOCIDÆ.					
Esox affinis (n.)		198	xxvii,*	1	*Lucius reticulatus.*
ravenelii (n.)		201		2	*Lucius americanus.*

* The references in the text under *Esox affinis* and *Esox ravenelii* are to plate xxviii, but the plate itself is numbered xxvii, while the plate numbered xxviii is referred to in the text as xxvii.

VII.

Unpublished Engraved Plates.

The last part of the first edition of the Ichthyology of South Carolina issued ended with page 184, in the midst of the description of the *Saurus fœtens* and of the dentition with the words "the palate;" the last plate was number "XXVII," containing the figures of two pikes, which were unnamed, but in the new and revised edition named *Esox affinis* and *Esox ravenelii.*

The copy of the first edition in the Library of Congress (Smithsonian deposit) is imperfect, pages 1 to 16, as well as 177 to 184, being wanting. I have not been able to ascertain definitely whether the description under the name *Centrarchus iridens* is applicable to that species (now known as *Centrarchus macropterus*) or not, but it probably is; the figure, however, is that of the entirely different *Enneacanthus obesus.*

Holbrook had ready for issue, all engraved, three plates, of which proofs were obtained by the Smithsonian Institution, and have been bound at the end of the imperfect copy of the first edition.

Two of these were numbered ("Pl. XXVIII" and "Pl. XXIX") and apparently had been drawn by Richard; they had been "printed by Tappan & Bradford, Boston." The other was unlettered and much more finely executed; the artist probably was Sonrel, and the plate had probably been drawn and engraved much later than the others.

On "Pl. XXVIII" are represented the Tailor Herring (*Pomolobus mediocris*) and Shad (*Alosa sapidissima*).

On "Pl. XXIX" are delineated the Menhaden (*Brevoortia tyrannus*) and Gizzard Shad (*Dorosoma cepedianum*).

On the unnumbered plate are ten figures representing various species of Cyprinodonts or Pœciliids, viz., *Fundulus majalis* (♂ and ♀), *Cyprinodon variegatus* (♂ and ♀), *Mollinesia latipinnis* (♂), *Fundulus chrysotus* (♂), *Gambusia affinis* (♂ and ♀), and *Heterandria formosa* (♂ and ♀).

The second part of the "Southern Ichthyology," supposed to be the only one published, is not in any of the great libraries, and the only copy I ever saw has disappeared from view. Recently I applied in vain for information respecting it to the Library of Congress, the Fish Commission, the Academy of Natural Sciences of Philadelphia, Mr. Samuel Garman of the Museum of Comparative Anatomy, and Dr. Anthony Woodward of the American Museum of Natural History.

Yrs respectfully
L. F. Pourtales

BIOGRAPHICAL MEMOIR

OF

LOUIS FRANÇOIS DE POURTALÈS.

1824-1880.

BY

ALEXANDER AGASSIZ.

READ BEFORE THE NATIONAL ACADEMY OF SCIENCES,
APRIL 22, 1881.

BIOGRAPHICAL MEMOIR OF L. F. DE POURTALÈS.

LOUIS F. DE POURTALÈS was born in Neuchâtel on the 4th of March, 1824, and died at Beverly Farms, on the 17th of July, 1880, in the fifty-seventh year of his age, sinking after a severe illness under an internal malady. The blow fell the more heavily upon his family and friends and upon his scientific colleagues, because his fine constitution, combined with a manly vigor of body and mind, had seemed to defy disease and to promise years of activity.

Educated as an engineer, he showed from boyhood a predilection for natural history. He was a favorite student of Professor Agassiz, and when only a lad of seventeen had shared his labors on the glacier of the Aar, being one of the party of Alpine explorers who, in 1840, made their home under the famous boulder known as the Hotel des Neuchâtelois. When his friend and teacher came to America in 1847, he accompanied him, and remained for some time with the little band of naturalists, who, first at East Boston and subsequently at Cambridge, shared his labors. In 1848 Pourtalès entered the United States Coast Survey, where his ability and indefatigable industry were at once recognized, and he remained attached to that branch of our public service for many years.

In 1851 he was engaged as assistant on the triangulation of the Florida Reef. While there he collected a number of Gephyreans and Holothurians which he described in the proceedings of the American Association for the Advancement of Science, together with a number of species observed by him while living at East Boston and assisting Professor Agassiz in the preparation of his text-book on zoölogy, afterwards published by him in conjunction with Dr. Gould. For this text-book Pourtalès prepared the greater number of the drawings. These descriptions and those of Dr. Gould and Dr. Simpson formed for a long time the only literature of the large number of Annelids and Holothurians, now so well known through the investigations of the Fish Commission along the Atlantic coast of the United States.

Thus prepared, Pourtalès became deeply interested in everything relating to the study of the bed of the ocean. Thanks to the enlightened support of the then superintendent of the Coast Survey, Professor Bache, and of his successors, Professor Peirce and Captain Patterson, he was enabled to devote his talents and industry to the comparatively new field of "Thalassography" and the biological investigations related to it. So interesting and valuable were the results obtained, not only as an aid to navigation, but in their wider bearing on the history of the Gulf Stream and on the distribution of animal life at great depths, that in 1866 he was sent out by Professor Peirce, then superintendent of the Coast Survey, to continue these investigations on a larger scale. The large collections of samples of sea-bottom accumulated by the different hydrographic expeditions of the United States Coast Survey were carefully examined by him; and the results, accompanied by a chart of the sea-bottom on the east coast of the United States, were published in Petermann's "Mittheilungen," in advance of their appearance in the Coast Survey reports.

From 1854 until his resignation as assistant in the United States Coast Survey, Mr. Pourtalès had charge of the field and office work of the tidal division. His reports to the superintendent of the United States Coast Survey, incorporated in the annual reports, short as they are, show the amount and value of his work. In addition to this tidal work, he was also at times assigned to special duty, as, for instance, at the magnetic station at Eastport. Previously to taking charge of the tidal division, he had been acting under the more direct supervision of the superintendent of the Coast Survey, either in connection with the tidal work or the calculation of longitudes.

While in Florida his attention was drawn to the habits of the Foraminifera, then little known, and his first papers on this subject were read at the meeting of the American Association for the Advancement of Science for 1850. They at once attracted attention, and after the death of Professor Bailey of West Point the larger series of samples from the sea-bottoms, collected by the officers of the Coast Survey, were submitted to him for examination. At that time the opinion of Forbes, that the depths of the sea were absolutely barren of life, was still generally accepted. Sharing this view with other naturalists, Pourtalès was nevertheless led to reconsider it in connection with his observations on his Foraminifera, many of which had been brought up from depths far below that considered by Forbes to be the limit of life. Did they belong there, or was their natural habitat, like that of others of their kind,* nearer the surface, and had they simply dropped to the bottom after death, or been gradually washed down from the reefs by the current? This question is discussed with much keenness of observation in his report on the Foraminifera collected by Craven and Maffitt. He inclined to believe that they actually lived where they were found, because the greater number of individuals in these specimens are brought up in perfect condition, notwithstanding the extreme delicacy of their shells. The faint pink color of the Globigerinæ, for instance, could scarcely be preserved had the specimens been transported from a distance, and the best argument in favor of their deeper habitat is found in the fact that the same species are found uninjured (and at great depths) as far north as New Jersey. It is, however, still most perplexing that the same species are also found living near Cuba and elsewhere in the West Indies under very different conditions of light and temperature.

He clearly saw that our ideas of the bathymetrical distribution of the higher invertebrates were to be greatly modified, for he says in one part of the report: "There are some delicate shells of mollusks from depths beyond five hundred fathoms where they were certainly living." He also called attention to the existence of green sand as one of the characteristic deep-sea formations of the present day. "A mixture in about equal proportions of Globigerinæ and black sand—probably green sand." In concluding he called attention to the importance for geologists of a knowledge of the habitat and distribution of Foraminifera, on account of their large share in the formation of rocks, at least in the Cretaceous and Tertiary periods.

While examining the samples of bottom collected by Commander Sands, he made the discovery that many specimens of Orbulina contained a young Globigerina, more or less developed, so that the two genera must be considered as probably two stages of alternate generation. He was also able, in some of the specimens collected by Commander Sands, to trace the successive

* Mr. Pourtalès, in 1867, observed a species of Globigerina floating on the surface off Havana.

changes of the Foraminifera into green sand from the most fresh-looking Foraminifera of various species until all trace of their origin was apparently lost.

In 1858 he gave a general report of his work on the Foraminifera of the bottoms collected by the various Gulf Stream expeditions up to that time. This was by no means the close of his studies in this direction, for he continued as a part of his duties of assistant on the Coast Survey to have charge of the collection of bottom deposits. The general results of his studies he published in Petermann's "Geographische Mittheilungen" for 1871, with a map showing the geographical and bathymetrical distribution of the different bottoms on the east coast of the Atlantic. Fitted as he was both by taste and early training for zoölogical studies, it was natural that the Coast Survey should look to him for an expansion of its biological work, and in 1867 he was assigned to the Coast Survey steamer *Corwin* in order to make such biological collections as would tend to elucidate the fauna of the bottom of the Gulf Stream between the Florida Keys and Cuba. The breaking out of yellow fever on board the steamer after a few casts had been taken put a stop to all further work for that season; but early in 1868 the campaign was opened again with such success—Acting Master Platt, U. S. N., commanding the Coast Survey steamer *Corwin*—that Mr. Pourtalès induced Professor Agassiz, who had become greatly interested in the extraordinary results of the second expedition, to join him in the *Bibb* for the third cruise in 1869. The second expedition, from the brilliant results obtained, may be said to have awakened general interest in the subject of the bathymetrical distribution of animal life. The discovery alone of the great range in depth of Rhizocrinus, from the Straits of Florida to the Loffoden Islands, opened a field of investigation, dimly foreshadowed, it is true, by the earlier dredgings of the older and younger Sars, and the wider bearing of which Lovén had anticipated in a paper read before a meeting of the Scandinavian naturalists as early as 1863.

In the Coast Survey Reports for 1867 and 1868 are to be found Pourtalès' first reports on the fauna of the Gulf Stream in the Straits of Florida. These reports were published with greater biological detail in the first volume of the Bulletins of the Museum of Comparative Zoölogy for 1867 and 1868.

The large and valuable collections made by Mr. Pourtalès in the Gulf Stream, as well as those made under his direction on board the *Hassler*, were deposited at the Museum of Comparative Zoölogy, Cambridge, and thence distributed as rapidly as possible to be worked up by specialists throughout the scientific world. To these were afterward added the results of the three *Blake* expeditions, which were, indeed, the natural continuation of the work initiated by Pourtalès. The collections thus sown broadcast have already borne a rich harvest in special reports upon Echinoderms, Corals, Crinoids, Foraminifera, Sponges, Annelids, Hydroids, Bryozoa, Mollusks, and Crustacea, prepared by the most eminent investigators of America and Europe, and published principally in the bulletins of the museum. They form a part of that series of international monographs to which Sir Wyville Thomson, following the liberal policy adopted and advised by the director of the museum, is making such generous contributions through the collections of the *Challenger*.

An examination of the characteristic deep sea Echinoderms, Sponges, and Corals showed at once the ancient characters of the types, while the similarity of the genera of Echini to those of the chalk, the discovery of representatives of the Infulasteridæ (Pourtalesia),* of Salenia, of Hemipedina, Conoclypus, and others, led the way to the theories of Thomson regarding the great antiquity of these forms and to the modern theories as to the formation of the chalk. The old view of Guyot and of Dana upon the great antiquity of continents and of oceanic basins received also a strong support from the data obtained in Mr. Pourtalès' dredgings. The specimens of bottom showed conclusively that we had not had in former geological times any deposits strictly corresponding to those now forming at the bottom of the ocean in great depths.

Mr. Pourtalès was indeed the pioneer of deep-sea dredging in America, and he lived long enough to see that these earlier expeditions had paved the way not only for similar English, French, and Scandinavian researches, but had led in this country to the *Hassler* and finally to the *Blake* expeditions, under the auspices of the Hon. Carlile Patterson, the present superintendent

*This genus is the representative of the most interesting family of Echini brought to light by deep-sea dredging. It was named in honor of Pourtalès in 1869.

of our Coast Survey. On the *Hassler* expedition, from Massachusetts Bay through the Straits of Magellan to California, he had entire charge of the dredging operations. Owing to circumstances beyond his control, the deep sea explorations of that expedition were not as successful as he anticipated.

At the death of his father, Mr. Pourtalès was left in an independent position, which allowed him to devote himself more completely than ever to his zoölogical studies. He resigned his official connection with the Coast Survey, and returned to Cambridge, where he became thenceforth identified with the progress of the Museum of Comparative Zoölogy. To Professor Agassiz his presence there was invaluable. In youth one of his favorite pupils, throughout life his friend and colleague, he now became the support of his failing strength.

Mr. Pourtalès reserved to himself the corals, Halcyonarians, Holothurians, and Crinoids of the different deep-sea dredging expeditions with which he was connected. A number of his papers on the deep-sea corals of Florida, of the Caribbean Sea, and of the Gulf of Mexico have appeared in the museum publications. The Crinoid memoirs published by him relate to a few new species of Comatulæ, and to the interesting genera Rhizocrinus and Holopus.

At the time of his death Mr. Pourtalès was engaged in the study of the Holothurians and the magnificent collection of Halcyonarians of the *Blake*. Unfortunately he had not advanced far enough in his preliminary work to make its completion possible, so that the Holothurians of the *Blake* will now be worked up with those of the *Challenger*, while the Halcyonarians must be left undetermined for the present, the Antipatharia alone having been finished.

His largest and most important work is his monograph on the deep-sea corals, published as one of the illustrated catalogues of the Museum of Comparative Zoölogy. This was published in 1871, and in it he describes the corals he collected in the years 1867–1869, while on the Coast Survey expeditions to explore the Gulf Stream. As an introduction to the memoir, we find a short *résumé* of the conditions of the floor of the Gulf Stream between Cuba, the Bahamas, and the Florida Keys, and a map with sections and other details showing the ground covered by the dredgings of Mr. Pourtalès. Throughout the memoir there are scattered most important general remarks on the affinities of the different families, the most interesting of which are those on the Rugosa and the Stylasteridæ. He also wrote for Appleton's Cyclopædia a number of articles on the Atlantic, Indian, and Pacific Oceans, on the Polar Seas, the Galapagos, the Straits of Magellan, Juan Fernandez, and Deep-Sea Dredging.

The titles of his memoirs indicate the range of his learning and his untiring industry. His devotion to science was boundless. A model worker, so quiet that his enthusiasm was known only to those who watched his steadfast labor, he toiled on year after year without a thought of self, wholly engrossed in his search after truth. He never entered into a single scientific controversy, nor even asserted or defended his claims to discoveries of his own which had escaped attention; but while modest to a fault and absolutely careless of his own position, he could rebuke in a peculiarly effective, though always courteous, manner ignorant pretensions or an assumption of infallibility.

Appointed keeper of the Museum of Comparative Zoölogy after the death of Professor Agassiz, he devoted a large part of his time to the administration of the museum affairs. Always at his post, he passed from his original investigations to practical details, carrying out plans which he had himself helped to initiate for the growth of the institution. As he had been the devoted friend of Professor Agassiz, he became to his son a wise and affectionate counsellor, without whose help in the last ten years the museum could not have taken the place it now occupies.

If he did not live to see the realization of his scientific hopes, he lived at least long enough to feel that their fulfillment is only a matter of time. He has followed Wyman and Agassiz, and like them has left his fairest monument in the work he has accomplished and the example he leaves to his successors.

The following are the principal publications of Mr. Pourtalès:

1850. On the Distribution of Foraminifera on the Coast of New Jersey, as shown by the Off-shore Soundings of the Coast Survey. Proc. Amer. Assoc. for Adv. of Sci., Charleston meeting, 1850.

On the Order of Succession of Parts in Foraminifera. Proc. Amer. Assoc. for Adv. of Sci., Charleston meeting, 1850. Also Amer. Jour. Sci. and Arts, 2d series, vol. II, 1851.

1851. On the Holothuridæ of the Atlantic Coast of the United States. Proc. Amer. Assoc. for Adv. of Sci., 1851, pp. 8.
A paper read in 1847 at meeting of Association of American Geolgists and Naturalists at Boston.

1851. On the Gephyrea of the Atlantic Coast of the United States. Proc. Amer. Assoc for Adv. of Sci., 1851, pp. 39.

1853. Extract from Letters of L. F. Pourtalès, Assistant in the Coast Survey, to the Superintendent upon the Examination of Specimens of Bottom obtained in the Exploration of the Gulf Stream by Lieutenants Commanding T. A. M. Craven and J. N. Maffitt, U. S. N. Coast Survey Report for 1853, and Proc. Amer. Assoc. for Adv. of Sci., Cleveland meeting, 1853.

1854. Tidal Reports.

1858. Report of Assistant L. F. Pourtalès on the Progress made in the Microscopical Examination of Specimens of Bottom from Deep-Sea Soundings. Coast Survey Report for 1858.

1858. On the Genera Orbulina and Globigerina. By L. F. Pourtalès. Amer. Jour. of Sci. and Arts, 2d series, vol. XXXVI, 1858.

1867. Report on the Fauna of the Gulf Stream in the Straits of Florida. By L. F. Pourtalès. Coast Survey Report for 1867.
Contributions to the Fauna of the Gulf Stream at Great Depths. By L. F. Pourtalès, Assistant U. S. Coast Survey. Bull. Mus. Comp. Zoöl., vol. I, No. 6. Cambridge, 1867, pp. 18.

1868. Contributions to the Fauna of the Gulf Stream at Great Depths (2d series). By L. F. Pourtalès, Assistant U. S. Coast Survey. Bull. Mus. Comp. Zoöl., vol. I, No. 7. Cambridge, 1868, pp. 22.

1868. Report of Assistant L. F. Pourtalès on Dredgings made in the Sea near the Florida Reef. Coast Survey Report for 1868.

1869. The Gulf Stream. Characteristics of the Atlantic Sea-Bottom off the Coast of the United States. By L. F. Pourtalès. Coast Survey Report for 1869.

1869. List of the Crinoids obtained on the Coasts of Florida and Cuba by the U. S. Coast Survey Gulf Stream Expeditions in 1867, 1868, and 1869. By L. F. Pourtalès, Assistant U. S. Coast Survey. Bull. Mus. Comp. Zoöl., vol. I, No. 11. Cambridge, 1869. pp. 4.

1869. List of Holothuridæ from the Deep-Sea Dredgings of the United States Coast Survey. By L. F. Pourtalès, Assistant U. S. Coast Survey. Bull. Mus. Comp. Zoöl., vol. I, No. 12. Cambridge, 1869, pp. 3.

1870. Der Boden des Golfstromes und der Atlantischen Küste Nord Amerika's. Von L. F. Pourtalès. Petermann's Geograph. Mittheilungen, 1870. Heft XI, pp. 5, 1 map.

1871. Deep-Sea Corals. By L. F. Pourtalès, Assistant U. S. Coast Survey. Illustrated Catalogue of the Mus. Comp. Zoöl., vol. II, No. 4. (Memoirs, vol. II, No. 4.) Cambridge, 1871, pp. 93, pls. 8.

1874. The Zoölogical Results of the Hassler Expedition. I. Echini, Crinoids, and Corals. By Alexander Agassiz and L. F. Pourtalès. Pp. 54, 15 woodcuts, pls. 10. II. Ophiuridæ and Astrophytidæ, including those dredged by the late Dr. Stimpson. By Theodore Lyman. Illustrated Catalogue of the Mus. Comp. Zoöl., No. 8. (Memoirs, vol. IV.) Cambridge, 1874, pp. 34, 4 woodcuts, pls. 5.

1875. Corals at the Galapagos Islands. By L. F. Pourtalès. Amer. Jour. of Sci. and Arts, 3d series, vol. X, 1875.

1876. Recent Corals from Tilibiche, Peru. By Alexander Agassiz and L. F. Pourtalès. Pp. 4, 1 plate, March, 1876.

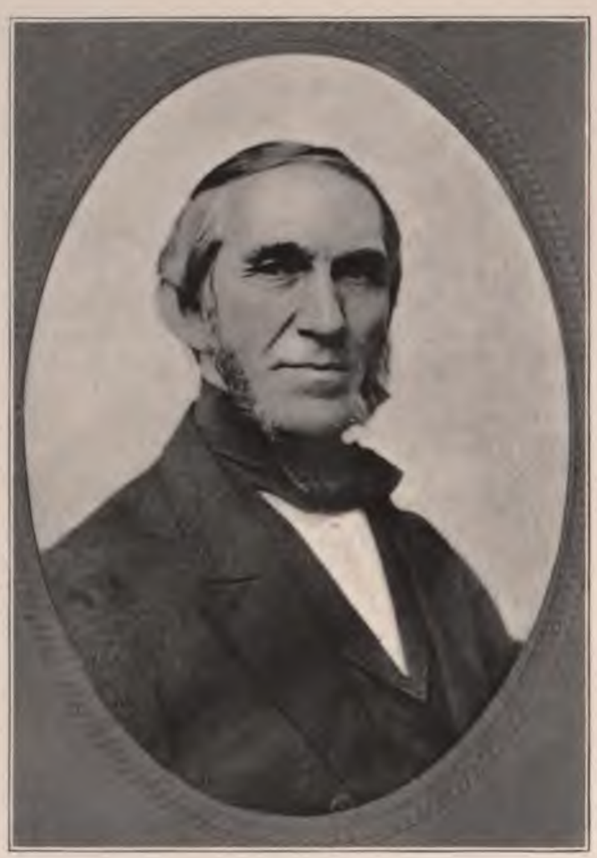
1878. Reports on the Dredging Operations U. S. Coast Survey Steamer *Blake*. II. Echini. By A. Agassiz. Corals and Crinoids. By L. F. Pourtalès. Ophiurans. By T. Lyman. Pp. 58, 11 plates, December 14, 1878.

1880. Report on the Results of Dredging, etc. VI. Report on the Corals and Antipatharia. By L. F. Pourtalès. Pp. 26, 3 plates, February, 1880.

1880. Report on the Florida Reefs. By Louis Agassiz. Accompanied by illustrations of Florida Corals, from drawings by A. Sonrel, Burkhardt, A. Agassiz, and Roetter. With an explanation of the plates, by L. F. Pourtalès. Published by permission of A. D. Bache and Carlile P. Patterson, Superintendent of the U. S. Coast Survey. Pp. 61, 23 plates, May, 1880. Mem. Mus. Comp. Zoöl., vol. VII, No. 1.

In Appleton's Encyclopædia the following articles:

Atlantic Ocean, vol. II.—Dredging (Deep-Sea), vol. VI.—Galapagos, vol. VII.—Indian Ocean, vol. IX.—Juan Fernandez, vol. IX.—Magellan, Straits of, vol. X.—Mediterranean Sea, vol. XI.—Pacific Ocean, vol. XII.—Polar Seas (geography), vol. XIII.



Aug. A. Gould

BIOGRAPHICAL MEMOIR

OF

AUGUSTUS ADDISON GOULD.

1805–1866.

BY

JEFFRIES WYMAN.

WITH ADDITIONS BY WILLIAM HEALEY DALL.

READ BEFORE THE NATIONAL ACADEMY OF SCIENCES,
APRIL 22, 1903.

BIOGRAPHICAL MEMOIR OF AUGUSTUS ADDISON GOULD.

The subject of this memoir descended from true pioneer stock and since heredity, especially of men of eminence, has a scientific as well as a personal interest, it is well to include here a brief notice of his lineage, derived from family documents.

The earliest ancestors noted are Zaccheus Gould, of Bovingdon, Herts, England, who emigrated to America about 1638 and died, aged 81, in 1670; and John Durant, or Duren, supposed French Huguenot, who emigrated in November, 1659, and in 1670 married Susannah Dalton.

One of the Durens, grandfather of our late associate, was a builder of note and designed a truss bridge which he erected over Pawtucket Falls, near Lowell, Mass., one of the earliest self-supporting bridges built in this country. His son, Nathaniel Gould Duren, was born in Bedford, Mass., in 1781, and when eleven years of age went to live with a maternal uncle at New Ipswich, N. H. This uncle, Nathaniel Gould, adopted his nephew, whose name was changed by legal process to Nathaniel Duren Gould. Young Gould, November 15, 1801, married Sally Andrews Prichard, of Welsh extraction, whose ancestors were among the earliest settlers of Old Rowley, now Boxford, Mass. This marriage was blessed with eight children, three of whom died in infancy. The second child and first survivor, born at New Ipswich, April 23, 1805, was AUGUSTUS ADDISON GOULD, the subject of this memoir.

The father was a musician, teacher of singing, and an engraver, noted for his beautiful penmanship, earnest piety, and cultivated mind, but, like most of his neighbors, in moderate circumstances financially. Those who remember his household in later days recall examples of plain living and good breeding such as in the early days of the United States were not infrequent in like situations. He was a man of deeply religious nature and a deacon in the congregation with which he worshiped.

Like many young men of that day, he turned his hand to many things. He taught school, vocal music, various wind and stringed instruments, led the church choir, managed a small farm, and in 1807 was appointed selectman of the village, a position which he held during his residence there. In 1815 he left the farm in charge of his family and proceeded to Boston, where he engaged in business, and from 1817 to 1820 was a member of the State Legislature. He taught in the grammar schools during the day, and in the evenings gave lessons in music, vocal or instrumental, training many church choirs and giving lessons to the students of Harvard College. He also urged the teaching of music in the public schools, and his own classes gave the first impulse to the public teaching of music now so general. Later in life his skill with the pen was utilized to engross the diplomas for the graduates of Harvard.

Augustus remained in New Ipswich during his boyhood, taking his part in the management of the little farm, set in a sort of amphitheater among the hills, and devoting a proportion of his time to study in the common school, where he gained the usual elements of an education.

At the age of fifteen he took the whole charge of the farm; nevertheless a part of his time was devoted to study, and some progress was made in the classics. By the careful husbanding of the odds and ends of time and a year's teaching at the local academy, he was prepared to enter college, and entered at Harvard in 1821. With his college life came a struggle, the forerunner of many such by which his strength was to be tried. He had already come to know something of the barrier which limited means had put between himself and the things he aspired to, and now this assumed larger proportions, such as to most persons would have been disheartening. College duties and exercises demanded his time, nevertheless his education must be paid for, and he must largely contribute towards earning the means; and so by strict economy, by performing various duties for which such students received compensation, and also by hard work in vacations and on those days which others gave to relaxation, he at length fought his way through, and attained to respectable rank.

In college he was noted among his classmates for industry, and it was there, too, that his taste for natural history began to show itself. He became familiar with the most of our native plants, and to the end of life never lost his love for them. After leaving college, he held the office of private tutor in Maryland, and at the same time began the study of medicine. The remainder of his studies were carried on in Boston, and the last year of them at the Massachusetts General Hospital as resident student. He was graduated in medicine in 1830, and at once began the practice of his profession, having given good grounds to his friends for expecting future eminence. But his struggles were not yet ended. Until his profession could yield him a support, he must go out of it, and did, to earn the necessaries of life. To this end he undertook burdensome tasks; one of them, the cataloguing and classification of the fifty thousand pamphlets in the library of the Boston Athenæum, was herculean, as any one may see who will take the trouble to look over the four large folio volumes he wrote out, monuments of his patient industry, for which he received fifty dollars. November 25, 1833, he married Harriet Cushing Sheafe, also of old colonial ancestry, connected with the well-known families of Loring, Cushing, and Quincy. This happy union, from which sprang ten children, of whom seven grew to maturity, was unbroken during the lifetime of Dr. Gould, whose widow survived him many years, dying May 14, 1893, at the age of eighty-two.

The study of natural history was nearer to his heart than all other pursuits, and to that he could always turn, and did, whenever he could command a few spare hours or moments to do so. He taught botany and zoölogy at Harvard for two years, and became a member of the Boston Society of Natural History soon after its organization. To the time he died, he labored for it, without stint. Here he was associated with Amos Binney, Storer, Wyman, and later with the elder Agassiz. For several years he was accustomed to rise at 4 o'clock a. m. and proceed to the rooms of the society to work on the collections, before the professional labors of the day were taken up. When his studies began to assume a systematic character, his first investigations were in the class of insects, of which, at one time, he had a large collection. Among his first published works was a monograph of the Cicindelidæ of Massachusetts, printed in 1834, and in 1840 he published an account of the American species of shells belonging to the genus Pupa, in regard to which he found much confusion. These shells are very small, and Mr. Say, who named all the species previously described, gave no figures, and consequently naturalists fell into error. "I have received from our best conchologists," Dr. Gould says, "a single species under four of the names that Mr. Say applied to as many different species." Dr. Gould then points out how, by the use of the microscope and a careful study of their minuter details, the classification of them might be improved. This paper was illustrated by about thirty figures carefully drawn by himself, with the aid of the microscope.

In 1841, he read before the Society a paper entitled "Results of an Examination of the Species of Shells of Massachusetts, and of their Geographical Distribution." This is the more noteworthy since the geographical distribution of animals had at that time attracted but little attention, and none amongst us. Now it involves one of the most important zoölogical problems. From his examination it appeared that of the shells found within the borders of the state, forty-two were of land or fresh-water habitat, and two hundred and three of marine origin. While some of the marine species are found on the transatlantic shores, he pointed out that of the air-breathing species a certain number were common to both continents, some of which had been imported.

Dr. Gould also points out in this paper the influence of shore outlines, and shows from a comparison of species, that Cape Cod, which stretches out into the sea in a curved direction some forty or fifty miles, forms to some species an impassable barrier. Of two hundred and three species, eighty do not pass to the south, and thirty have not been found to the north. In the same paper he calls attention to the importance of the fact that certain species appear and disappear suddenly, and of the necessity, in order to construct a correct catalogue of the shells of any region, of extending observations through a series of years, a consideration which many naturalists, even of the present day, might profit by. In the spring of 1830, *Osteodesma* was strewed upon Chelsea Beach in great number, and of very large size, but had never been observed there before, and has seldom been seen since. *Cyprina islandica*, *Solemya velum*, *Venus gemma*, and *Margarita arctica* also present instances of periodicity at long intervals. During the winter of 1838–'39, *Yoldia thraciæformis* was frequently found in the stomachs of the sand-dab, but search for them since was for a long time almost fruitless.

One of the first results of the joint action of the members of the Boston Society of Natural History, and of which it has more reason to be proud than any other, was the part taken by some of them in the series of admirable reports on the natural history of the State of Massachusetts presented to the General Court in compliance with a legislative enactment. The report on the Trees was by Mr. George B. Emerson, then President of the Society; that on Fishes, by Dr. D. H. Storer; that on Insects Injurious to Vegetation, by Dr. T. W. Harris, and that on Invertebrate Animals, excepting insects, by Dr. Gould. They at once gained for their authors widespread reputation.

The Mollusks were Dr. Gould's favorite subjects for study, and his attention was chiefly given to them. Up to this time few, if any, attempts had been made to give as complete a zoölogical survey as practicable of any particular region of the United States. As regards the Mollusks, the descriptions of Say, Conrad, and others, pioneers in conchology, pertained more to the Middle and Western States than to New England. Their writings were fragmentary and scattered through the narratives of travels, journals of science, and even newspapers. It was no small labor, therefore, to become acquainted, merely as a preparation for his task, with the writings of his predecessors. To make his report as complete as possible, and to ascertain what changes in the classification of Mollusks recent important progress growing out of the study of them would indicate, he opened correspondence for information and exchanges with European naturalists interested in the same branch of study, who obligingly and courteously lent their aid, and out of this correspondence grew up long continued friendships.

The report fills a volume of nearly four hundred pages, illustrated by more than two hundred figures skillfully drawn from nature by himself. "Every species described," he says, "indeed almost every species mentioned, has passed under my own eye. The descriptions of species previously known have been written anew, partly that they might be more minute in particulars, and partly with the hope of using language somewhat less technical than is ordinarily employed by scientific men." The number of species described was about two hundred and seventy-five of Mullusks and nearly one hundred of Crustaceans and Radiates.

This served as a manual of New England shells excellent in every way, and which had much to do with interesting in this subject many students with a taste for natural history, some of whom, among whom may be mentioned the late Dr. William Stimpson, attained to eminence.

As a contribution to zoölogical science, this report at once gave him an honorable name among the naturalists of Europe and America.*

Dr. Gould edited the admirable work entitled "The Terrestrial Air-breathing Mollusks of the United States," prepared, but left unfinished at the time of his death, by his intimate friend, Dr. Amos Binney, formerly the respected president of the Society of

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* The following translation of a letter received by Dr. Gould from Prof. Louis Agassiz not only illustrates the manner in which the Report on the Invertebrates of Massachusetts was received by his cotemporaries, but has an intrinsic historic interest for American naturalists:

MY DEAR SIR:

Since I have received your excellent history of the invertebrates of Massachusetts, which I owe to the kindness of the Boston Society of Natural History, I have several times wished to write you to thank you for the pleasure and instruction which I have derived from reading this faunal monograph. If I have not written sooner it is because I have long contemplated making a journey to the United States, and in the uncertainty in which I have been until lately in regard to the matter I did not feel that I should trouble you with these plans, which I have ardently desired to carry out, and on which I have need of your advice and counsel.

Today my plan is decided on, thanks to the munificence of His Majesty the King of Prussia, who, on the recommendation of Baron von Humboldt, has assumed the expense of this journey. I can leave in the course of the summer, probably early in July. My intention is to remain at least a year and a half in your country, with the special object of studying your recent and fossil faunas in order to compare them with those of Europe. I certainly shall not pretend to travel for the sake of making new zoölogical or paleontological discoveries in the United States. The American savants are too active and intelligent to leave such a possibility to a European. My end will have been accomplished when I have learned to recognize all that your countrymen have done, have examined with care your public and private museums, and have gathered materials sufficient to establish the immediate comparisons which I need to make between your species and ours—above all, those which are reputed to be identical. I hope also to establish harmony between generic and specific names employed often in very different senses by you and

Natural History, and whose name is held in grateful remembrance, not only for his contributions to science, but for the munificent bequest which fills so large a space on the shelves of the library of that institution.

The plan of this work was broad and philosophical, passing far out of the region of generic and specific technicalities into the wider subjects of the principles of classification, of the geographical distribution of genera and species, and the causes influencing it, of zoölogical foci or points of origin, geological relations, habits, faculties, and anatomical structure. Its incomplete state, the fact that many of the species collected by Dr. Binney in the southwestern States and Texas had not been described by him

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by us, and to render, according to the law of priority, to each one his due, following the date of his publications.

The great superiority of your work on the Mollusks of Massachusetts, which has struck me especially by the precision and the development of the descriptions of species which it contains, has led me to address myself to you to know where you think I might most conveniently establish myself on the coast to study those soft animals which must be examined on the spot, since they can only be imperfectly preserved in alcohol or otherwise. I had thought of making different stations—at first north of Boston, then in the vicinity of Boston, then near New York, and finally further south, in the vicinity of Charleston; but before taking measures to carry out this project I should like to know if one can easily establish one's self for some weeks in the immediate neighborhood of the sea, in the places most favorable for study, like those on our coast of Normandy, or whether it will be necessary to resign one's self to living in town and making excursions to the shore, which is always accompanied by a considerable loss of time. Naturalist from childhood, habituated to all kinds of fatigue and privation, it is not discomfort which I should find an obstacle, but rather the loss of precious time. You will infinitely oblige me by giving me some information on this subject. Perhaps you will also have the goodness to enquire of Dr. Storer about the fishes which he seems to know so well, for it will be very useful to be directed from the moment of my arrival. I am also going to write to Mr. Haldeman, whose work on the fresh-water shells I much appreciate, to ask of him suggestions for journeys in the interior. * * *

Will you tell me what you think of this plan and excuse me for addressing myself thus informally to you, for I have recognized in your fine work so liberal a spirit that I have believed I could write to you as to a colleague. Accept, my dear sir, the assurance of my high consideration.

L. Agassiz.

Neuchâtel en Suisse, *May 6, 1845.*

up to the time of his death, and changes made necessary by more recent observations, rendered the editorship of this work no sinecure. No one could be found more fit for the task, or more worthy to bring before the world the labors of a deceased friend.

In 1848 he was associated with Prof. Louis Agassiz in the preparation of the Principles of Zoölogy, a small volume intended for use in schools.

His largest and most important contribution to natural history was the description of the shells of the United States Exploring Expedition. This was prepared under somewhat embarrassing circumstances. The collection was not made by himself, but by the late Capt. James P. Couthouy, U. S. N., well remembered as a most zealous and active naturalist. Captain Couthouy had drawn up full notes on the external characters of the soft parts, habits, geographical distribution, and on other important points. Before the voyage was completed he left the expedition, but the notes and collections were sent to Washington. The former were unaccountably lost, and no trace of them was found. The collections, when they came into the hands of the Navy Department, were unpacked by incompetent hands, the arrangement of them disturbed, labels in many cases lost, and the whole thrown more or less into confusion. Dr. Gould was called upon to save the wreck, but in accepting the task was obliged to submit to various arbitrary restrictions, and to leave undone many things he deemed of much importance. Fully appreciating the value of a knowledge of the internal structure of the animals, and knowing too well the folly of attempting to find all the characters for a zoölogical description in the shell alone, he expresses his regret at the outset that full dissections and delineations of the internal features had not been directed or allowed. This was all the more to be regretted, since there was a great abundance of material for the required investigations.

Agreeably to his instructions, the work is almost wholly confined to generic and specific descriptions. In the introduction, however, he presents several generalizations of importance. By a careful comparison he shows that Mollusca are confined generally to definite districts or areas. Descriptive writers have frequently given support to opposite views, and have fallen into error from not having taken proper care to ascertain the locality from which certain species came, a determination which is now considered of prime importance. Shells purchased in the Hawaiian Islands have been described as denizens of these islands, notwithstanding they may have been carried there from far-off places. New England shells which have been sent to the western coast of America have been known to come back in the way of exchanges as natives of the Pacific shores. Errors have also been committed by attempting to decide the identity of species from distant places by the shell alone, when observation has proved this in many cases impossible. When such and other sources of error are eliminated, the number of apparently identical species from widely different sources rapidly diminishes. In fact, the doctrine of the faunal limitation of animals meets with so few exceptions that we admit it is an axiom in zoölogy, he says, that species resembling one another from widely diverse localities, especially if a continent intervenes, and if no plausible means of communication can be assigned, are different until their identity can be proved. It is true that some species are more or less cosmopolite, as the Cypreas, and, as at present understood, do not appear to be closely limited, while others become cosmopolite by transportation, as certain Helices, which attach themselves to the water casks of ships or imported plants and thus are carried around the world.

Another general consideration, and closely related to geographical distribution, grows out of the fact that the shells from definite regions have peculiarities of external form and color, of what may be called style, just as have the human races from different parts of the world. Thus, he says, we distinguish the loose, colorless structure of the northern marine species, the stony, corroded, and livid New Zealanders, and the polished and absolutely perfect specimens from the coral seas.

Another generalization illustrated by the ample stores of the expedition is the occurrence of analogous species in coördinate regions, though the species themselves are absolutely distinct, in confirmation of which he gives a list of some thirty-two species found on the eastern and western coasts of the United States.

Lastly, it is shown, by a careful comparison of the land shells of the Pacific islands, how one is helped in drawing inferences as to the lands which once occupied the area of the Pacific, and how, in consequence of their submergence, their mountain peaks, which now alone project above the surface of the water, constitute these islands. The Samoa and Friendly Islands give evidence of such relation in having identical species.

The Otia Conchologica was the last of his conchological books, but this was merely a reprint in a condensed form of descriptions of species of shells previously published separately. Besides the works already mentioned, there is a long catalogue of communications made to the Boston Society of Natural History, and which is appended to this notice, which may be referred to as showing that he did not allow himself to become a mere specialist, but kept his mind awake to the relation of individual forms to higher and more general truths. On the incorporation of the National Academy of Sciences, in 1863, Dr. Gould became one of the charter members.

We must not forget that Dr. Gould was a member of the medical profession, and that his time was of necessity chiefly devoted to this, while the scientific labors we have been considering were the yield of spare moments made useful. He was an active member of the medical societies of this city and of the state, and held offices of trust in them. The Massachusetts Medical Society conferred on him the honors which it has to bestow upon its fellows. In 1855 he delivered the annual address, which was marked for the soundness of its views and the characteristic clearness and elegance with which they were presented. He took for his text the advice of Harvey to the Royal College of Physicians of London, when he founded the annual oration which bears his name, and in which, among other things, he enjoins upon the orator "an exhortation to the members to study and search out the secrets of nature by the way of experiment." Dr. Gould was elected president of the Society in 1864, and his term of office ended within a few months of his death. He was for several years one of the consulting physicians of the Massachusetts General Hospital, was an efficient member of the Boston Society for Medical Improvement, where he often communicated valuable observations, and took an active part in its discussions. He labored much and long in preparing the vital statistics of the state from the official returns.

At one of the meetings of the National Academy of Sciences he presented an important paper on the distribution of certain diseases, especially consumption, in reference to the hygienic choice of a location for the cure of invalid soldiers. The census of 1860 gave the means of arriving at a definite result, and of showing that the mortality from the disease mentioned was greatest in the north, and diminished southwards almost as regularly as the states could be called. It causes about 29 per cent. of all the deaths in Maine, and only 3 per cent. of those in Arkansas. Infirmaries established with the idea of sending patients to those regions where the disease to be treated is presented in its mildest aspect must be far more successful than the ordinary method of mingling together invalids suffering from all sorts of infirmities.

As a citizen, Dr. Gould made a principle of going out of the ordinary routine of life to lend a helping hand wherever it was desired and he could. He served the public in many capacities—in the religious society of which he was from early life a member, and in the public schools, where he took an active interest in all attempts to improve the ways and means of instruction. He from time to time gave public lectures, and although in this capacity he could not be said to be brilliant or highly accomplished, yet his unostentatious manner and simplicity, his knowledge of his subject and hearty interest in it, always gained him attentive listeners, who went away instructed.

What can be said by way of acknowledgment of the unrequited work he did for the Boston Society of Natural History; of his services in the formation of the cabinet, and in promoting the interests of the society in a hundred ways; above all, in the drudgery which only ended with his life, his aid in preparing for the press and in superintending the publication of the various volumes printed by the society, from the first to the last?

What has now been said relates only to some of the more tangible features of his principal works, leaving out of sight the industry, the critical acumen, the tact and perceptive power required to prepare them. This we can never fully appreciate, nor the difficulties under which his work was done. One could only do this by watching his patient studies in the intervals of professional calls, or as he labored at early dawn or late at night in the hours stolen from sleep. Though often an invalid, the sickness must have been irksome indeed which could restrain him from his accustomed work.

In his temperament he was genial, and drew friends around him, retaining the old and attracting new. He came to the social gathering with joyous face and kindly feelings. His love for natural scenery was genuine and hearty, and whatever personal enjoyment came from this source, it was always enhanced if others partook of it with him. There are too many naturalists who stand in the presence of nature all their days, but see her not. To them the world offers nothing but the forms they would technically describe and arrange in their cabinets. Take away this object and all becomes a waste, for they are neither warmed nor enlivened by the world around them. Not so with our associate. No one toiled more industriously than he over individual forms and specific descriptions; but, all this aside, every aspect of nature touched him to the innermost. Those who have been intimate with him know how his face would light up while in the presence of the least as well as of the greatest natural objects—the flower of a day or the sturdy tree that had known its centuries of life, the quiet or the grander scenes of the world. His emotions were not those of an enthusiast, but rather came of a clear perception and calm contemplation of the things around him and of his own responsive nature.

He was a deeply religious man, and for more than thirty years a consistent member of the Baptist communion, exemplifying his piety by his life rather than by his conversation.

Dr. Gould was tall and spare, with dark hair and dark gray eyes, his countenance full of character and benevolence. When greeting others in whom he was interested, especially young students, his face had a winning expression not to be forgotten.

His life, all too poorly and inadequately represented in this sketch, was throughout a consistent one, and to the end each day was full to the round. He was still endeavoring to improve what had been done before, and looking forward to the accomplishment of new and better ends, when suddenly it was closed. He had been less well than usual. On the afternoon of September 14, 1866, he manifested the usual symptoms of an attack of Asiatic cholera, soon after fell into a state of collapse, and on the following morning, just before the dawn, he died. His remains lie in the beautiful cemetery of Mount Auburn, near Boston.

NOTE.—The foregoing biographical sketch was read by Dr. Jeffries Wyman before the Boston Society of Natural History in 1867, and as originally written appears in their Proceedings, vol. xi, pp. 188–197, July, 1867. Some changes and additions suggested by the family of Dr. Gould, or supplying information which seemed proper to be included, yet which had been omitted in the original paper, serve to make the present recension more complete. A good portrait of Dr. Gould appeared in the Annual of Scientific Discovery for 1861, and was afterward reprinted in the American Journal of Conchology, vol. i, part iv, in 1865.

LIST OF SOCIETIES AND INSTITUTIONS WITH WHICH DR. GOULD WAS ASSOCIATED AND THE DATES OF HIS ELECTION OR SERVICE.

1828. House physician, Massachusetts General Hospital, afterward serving as physician (1855) and consulting physician (1863).

1832. Member Massachusetts Medical Society. Served as orator at the anniversary meeting of 1855, and President, 1864-'66.

1832. Member Boston Society of Natural History and one of its curators.

1836. Corresponding member Connecticut Natural History Society.

1837. Corresponding member Rhode Island Natural History Society.

1837. Corresponding member of the Natural History Society of Athens.

1840. Corresponding member of the Academy of Natural Sciences, Philadelphia; of the Literary and Historical Society of Quebec; of the National Institute, Washington, and the American Statistical Association.

1841. Member of the American Academy of Arts and Sciences, Boston, Mass.

1842. Corresponding member Kongl. Norske Videnskabers Selskab, Copenhagen, and of the Imperial Mineralogical Society of St. Petersburg, Russia.

1844. Member of the Natural History Society of Lynn, Mass.

1844. Corresponding member Kongl. Nordiske Oldskrift-Selskab, Copenhagen.

1846. Corresponding member Lyceum of Natural History of New York.

1847. Member Essex County (Mass.) Natural History Society.

1848. Member of the Phi Beta Kappa Society.

1849. Member of the American Medical Association.

1849. Member of the American Philosophical Society of Philadelphia.

1856. Member Massachusetts Horticultural Society.

1858. Elected President of the Suffolk District Medical Society, in which capacity he served until 1860.

1863. Charter member of the National Academy of Sciences.

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BIOGRAPHICAL MEMOIR

OF

HENRY AUGUSTUS ROWLAND.

1848-1901.

BY

THOMAS C. MENDENHALL.

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BIOGRAPHICAL MEMOIR OF HENRY A. ROWLAND.

In reviewing the scientific work of Professor Rowland one is most impressed by its originality. In quantity, as measured by printed page or catalogue of titles, it has been exceeded by many of his contemporaries; in quality it is equaled by that of only a very, very small group. The entire collection of his important papers does not exceed thirty or forty in number and his unimportant papers were few. When, at the unprecedentedly early age of thirty-three years, he was elected to membership in the National Academy of Sciences, the list of his published contributions to science did not contain over a dozen titles; but any one of not less than a half-dozen of these, including what may properly be called his very first original investigation, was of such quality as to fully entitle him to the distinction then conferred.

Fortunately for him, and for science as well, he lived during a period of almost unparalleled intellectual activity, and his work was done during the last quarter of that century to which we shall long turn with admiration and wonder. During these twenty-five years the number of industrious cultivators of his own favorite field increased enormously, due in large measure to the stimulating effect of his own enthusiasm, and while there was only here and there one possessed of the *divine afflatus* of true genius, there were many ready to labor most assiduously in fostering the growth, development, and final fruition of germs which genius stopped only to plant. A proper estimate of the magnitude and extent of Rowland's work would require, therefore, a careful examination, analytical and historical, of the entire mass of contributions to physical science during the past twenty-five years, many of his own being fundamental in character and far-reaching in their influence upon the trend of thought, in theory and in practice. But it was quality, not quantity, that he himself most esteemed in any performance; it was quality that always commanded his admiration or excited him to keenest criticism; no one recognized more quickly than he a real gem, however minute or fragmentary it might be, and by quality rather than by quantity we prefer to judge his work today, as he would himself have chosen.

Rowland's first contribution to the literature of science took the form of a letter to *The Scientific American*, written in the early autumn of 1865, when he was not yet seventeen years old. Much to his surprise this letter was printed, for he says of it, "I wrote it as a kind of joke, and did not expect them to publish it." Neither its humor nor its sense, in which it was not lacking, seems to have been appreciated by the editor, for by the admission of certain typographical errors he practically destroyed both. The embryo physicist got nothing but a little quiet amusement out of this, but in a letter of that day he declares his intention of some time writing a sensible article for the journal that so unexpectedly printed what he meant to be otherwise. This resolution he seems not to have forgotten, for nearly six years later there appeared in its columns what was, as far as is known, his second printed paper and his first serious public discussion of a scientific question. It was a keen criticism of an invention which necessarily involved the idea of perpetual motion, in direct conflict with the great law of the Conservation of Energy which Rowland had already grasped. It was, as might be expected, thoroughly well done, and received not a little complimentary notice in other journals. This was in 1871, the year following that in which he was graduated as a civil engineer from the Rensselaer Polytechnic Institute, and the article was written while in the field at work on a preliminary railroad survey. A year later, having returned to the Institute as instructor in physics, he published in the *Journal of the Franklin Institute* an article entitled "Illustrations of Resonances and Actions of a Similar Nature," in which he described and discussed various examples of resonance or "sympathetic" vibration. This paper, in a way, marks his admission to the ranks of professional students of science and may be properly considered as his first formal contribution to scientific literature; his last was an exhaustive article on spectroscopy, a subject of which he, above all others, was master, prepared for a new edition of the Encyclopædia Britannica, not yet published. Early in 1873 the *American Journal of Science* printed a brief note by Rowland on the spectrum of the Aurora, sent in response to a kindly and always appreciated letter from Professor George F. Barker, one of the editors of that journal. It is interesting as marking the beginning of his optical work. For a year, or perhaps for several years, previous to this time, however, he had been busily engaged on what proved to be, in its influence upon his future career, the most important work of his life. To climb the ladder of reputation and success by simple, easy steps might have contented Rowland, but it would have been quite out of harmony with his bold spirit, his extraordinary power of analysis and his quick recognition of the relation of things. By the aid of apparatus entirely of his own construction and by methods of his own devising, he had made an investigation both theoretical and experimental of the magnetic permeability and the maximum magnetization of iron, steel, and nickel, a subject in which he had been interested in his boyhood. On June 9, 1873, in a letter to his sister, he says: "I have just sent off the results of my experiments to the publisher and expect considerable from it; not, however, filthy lucre, but good, substantial reputation." What he did get from it at first was only disappointment and discouragement. It was more than once rejected because it was not understood, and finally he ventured to send it to Clerk Maxwell, in England, by whose keen insight and profound knowledge of the subject it was instantly recognized and appraised at its full value. Regretting that the temporary suspension of meetings made it impossible for him to present the paper at once to the Royal Society, Maxwell said he would do the next best thing, which was to send it to the *Philosophical Magazine* for immediate publication, and in that journal it appeared in August, 1873, Maxwell himself having corrected the proofs to avoid delay. The importance of the paper was promptly recognized by European physicists, and abroad, if not at home, Rowland at once took high rank as an investigator.

In this research he unquestionably anticipated all others in the discovery and announcement of the beautifully simple law of the magnetic circuit, the magnetic analogue of Ohm's law, and thus laid the foundation for the accurate measurement and study of magnetic permeability, the importance of which, both in theory and practice during recent years, it is difficult to overestimate. It has always seemed to me that when consideration is given to his age, his training, and the conditions under which his work was done, this early paper gives a better measure of

Rowland's genius than almost any performance of his riper years. During the next year or two he continued to work along the same lines in Troy, publishing not many, but occasional, additions to and developments of his first magnetic research. There was also a paper in which he discussed Kohlrausch's determination of the absolute value of the Siemens unit of electrical resistance, foreshadowing the important part which he was to play in later years in the final establishment of standards for electrical measurement.

In 1875, having been appointed to the professorship of physics in the Johns Hopkins University, the faculty of which was just then being organized, he visited Europe, spending the better part of a year in the various centres of scientific activity, including several months at Berlin in the laboratory of the greatest continental physicist of his time, Von Helmholtz. While there he made a very important investigation of the magnetic effect of moving electrostatic charges, a question of first rank in theoretical interest and significance. His manner of planning and executing this research made a marked impression upon the distinguished director of the laboratory in which it was done, and, indeed, upon all who had any relations with Rowland during its progress. He found what Von Helmholtz himself had sought for in vain, and when the investigation was finished in a time which seemed incredibly short to his more deliberate and painstaking associates, the director not only paid it the compliment of an immediate presentation to the Berlin Academy, but voluntarily met all expenses connected with its execution.

The publication of this research added much to Rowland's rapidly growing reputation, and because of that fact, as well as on account of its intrinsic value, it is important to note that his conclusions have been held in question, with varying degrees of confidence, from the day of their announcement to the present. The experiment is one of great difficulty and the effect to be looked for is very small, and therefore likely to be lost among unrecognized instrumental and observational errors. It was characteristic of Rowland's genius that with comparatively crude apparatus he got at the truth of the thing in the very start. Others who have attempted to repeat his work have not been uniformly successful, some of them obtaining a wholly negative result, even when using apparatus apparently more complete and effective than that first employed by Rowland. Such was the experience of Lecher in 1884, but in 1888 Roentgen confirmed Rowland's experiments, detecting the existence of the alleged effect. The result seeming to be in doubt, Rowland himself, assisted by Hutchinson, in 1889 took it up again, using essentially his original method but employing more elaborate and sensitive apparatus. They not only confirmed the early experiments, but were able to show that the results were in tolerably close agreement with computed values. The repetition of the experiment by Himstedt in the same year resulted in the same way, but in 1897 the genuineness of the phenomenon was again called in question by a series of experiments made at the suggestion of Lippmann, who had proposed a study of the reciprocal of the Rowland effect, according to which variations of a magnetic field should produce a movement of an electrostatically charged body. This investigation, carried out by Crémieu, gave an absolutely negative result, and because the method was entirely different from that employed by Rowland, and therefore unlikely to be subject to the same systematic errors, it naturally had much weight with those who doubted his original conclusions. Realizing the necessity for additional evidence in corroboration of his views, in the fall of the year 1900, the problem was again attacked in his own laboratory and he had the satisfaction, only a short time before his death, of seeing a complete confirmation of the results he had announced a quarter of a century earlier, concerning which, however, there had never been the slightest doubt in his own mind. It is a further satisfaction to his friends to know that a very recent investigation at the Jefferson Physical Laboratory of Harvard University, in which Rowland's methods were modified so as to meet effectively the objections made by his critics, has resulted in a complete verification of his conclusions.

On his return from Europe, in 1876, his time was much occupied with the beginning of the active duties of his professorship, and especially in putting in order the equipment of the laboratory over which he was to preside, much of which he had ordered while in Europe. In its arrangement great, many of his friends thought undue, prominence was given to the workshop, its machinery, tools, and especially the men who were to be employed in it. He planned wisely, however, for he meant to see to it that much, perhaps most, of the work under his direction should be in the nature of original investigation, for the successful execution of which a well manned and equipped workshop is worth more than a storehouse of apparatus already designed and used by others.

He shortly found leisure, however, to plan an elaborate research upon the Mechanical Equivalent of Heat, and to design and supervise the construction of the necessary apparatus for a determination of the numerical value of this most important physical constant, which he determined should be exhaustive in character and, for some time to come at least, definitive. While this work lacked the elements of originality and boldness of inception by which many of his principal researches are characterized, it was none the less important. While doing over again what others had done before him, he meant to do it, and did do it, on a scale and in a way not before attempted. It was one of the *great* constants of nature, and, besides, the experiment was one surrounded by difficulties so many and so great that few possessed the courage to undertake it with the deliberate expectation of greatly excelling anything before accomplished. These things made it attractive to Rowland.

The overthrow of the materialistic theory of heat, accompanied as it was by the experimental proof of its real nature, namely, that it is essentially molecular energy, laid the foundation for one of those two great generalizations in science which will ever constitute the glory of the nineteenth century. The mechanical equivalent of heat, the number of units of work necessary to raise one pound of water one degree in temperature, has with much reason been called the Golden Number of that century. Its determination was begun by an American, Count Rumford, and finished by Rowland nearly a hundred years later. In principle the method of Rowland was essentially that of Rumford. The first determination was, as we now know, in error by nearly 40 per cent.; the last is probably accurate within a small fraction of 1 per cent. Rumford began the work in the ordnance foundry of the Elector of Bavaria at Munich, converting mechanical energy into heat by means of a blunt boring tool in a cannon surrounded by a definite quantity of water, the rise in temperature of which could be measured. Rowland finished it in an establishment founded for and dedicated to the increase and diffusion of knowledge, aided by all the resources and refinements in measurement which a hundred years of exact science had made possible. As the mechanical theory of heat was the germ out of which grew the principle of the conservation of energy, an exact determination of the relation of work and heat was necessary to a rigorous proof of that principle, and Joule, of Manchester, to whom belongs more of the credit for this proof than to any other one man, or perhaps to all others put together, experimented on the mechanical equivalent of heat for more than forty years. He employed various methods, finally recurring to the early method of heating water by friction, improving on Rumford's device by creating friction in the water itself. Joule's last experiments were made in 1878, and most of Rowland's work was done in the year following. It excelled that of Joule, not only in the magnitude of the quantities to be observed, but especially in the greater attention given to the matter of thermometry. In common with Joule and other previous investigators, he made use of mercury thermometers, but this was only for convenience, and they were constantly compared with an air thermometer, the results being finally reduced to the absolute scale. By experimenting with water at different initial temperatures he obtained slightly different values for the mechanical equivalent of heat, thus establishing beyond question the variability of the specific heat of water. Indeed, so carefully and accurately was the experiment worked out that he was able to draw the variation curve and to show the existence of a minimum value at 30 degrees C.

This elaborate and painstaking research, which is now classical, was everywhere awarded high praise. It was published in full by the American Academy of Arts and Sciences with the aid of a fund originally established by Count Rumford, and in 1881 it was crowned as a prize essay by the Venetian Institute. Its conclusions have stood the test of twenty years of comparison and criticism.

In the meantime, Rowland's interest had been drawn, largely perhaps through his association with his then colleague, Professor Hastings, towards the study of light. He was an early and able exponent of Maxwell's magnetic theory and he published important theoretical discussions of electro-magnetic action. Recognizing the paramount importance of the spectrum as a key to the solution of problems in ether physics, he set about improving the methods by which it was produced and studied, and was thus led into what will probably always be regarded as his highest scientific achievement.

At that time, the almost universally prevailing method of studying the spectrum was by means of a prism or a train of prisms. But the prismatic spectrum is abnormal, depending for its character largely upon the material made use of. The normal spectrum as produced by a grating of fine wires or a close ruling of fine lines on a plane reflecting or transparent surface had been known for nearly a hundred years, and the colors produced by scratches on polished surfaces were noted by Robert Boyle, more than two hundred years ago. Thomas Young had correctly explained the phenomenon according to the undulatory theory of light, and gratings of fine wire and, later, of rulings on glass were used by Fraunhofer, who made the first great study of the dark lines of the solar spectrum. Imperfect as these gratings were, Fraunhofer succeeded in making with them some remarkably good measures of the length of light waves, and it was everywhere admitted that for the most precise spectrum measurements they were indispensable. In their construction, however, there were certain mechanical difficulties which seemed for a time to be insuperable. There was no special trouble in ruling lines as close together as need be; indeed, Nobert, who was long the most successful maker of ruled gratings, had succeeded in putting as many as a hundred thousand in the space of a single inch. The real difficulty was in the lack of uniformity of spacing, and on uniformity depended the perfection and purity of the spectrum produced. Nobert jealously guarded his machine and method of ruling gratings as a trade secret, a precaution hardly worth taking, for before many years the best gratings in the world were made in the United States. More than thirty years ago an amateur astronomer, in New York City, a lawyer by profession, Lewis M. Rutherfurd, became interested in the subject and built a ruling engine of his own design. In this machine the motion of the plate on which the lines were ruled was produced at first by a somewhat complicated set of levers, for which a carefully made screw was afterwards substituted. Aided by the skill and patience of his mechanician, Chapman, Rutherfurd continued to improve the construction of his machine until he was able to produce gratings on glass and on speculum metal far superior to any made in Europe. The best of them, however, were still faulty in respect to uniformity of spacing, and it was impossible to cover a space exceeding two or three square inches in a satisfactory manner.

When Rowland took up the problem he saw—as, indeed, others had seen before him—that the dominating element of a ruling machine was the screw by means of which the plate or cutting tool was moved along. The ruled grating would repeat all of the irregularities of this screw, and would be good or bad just as these were few or many. The problem was, then, to make a screw which would be practically free from periodic and other errors, and upon this problem a vast amount of thought and experiment had already been expended. Rowland's solution of it was characteristic of his genius. There were no easy advances through a series of experiments in which success and failure mingled in varying proportions. "Fire and fall back" was an order which he neither gave nor obeyed, capture by storm being more to his mind. He was by nature a mechanician of the highest type, and he was not long in devising a method for removing the irregularities of a screw, which astonished everybody by its simplicity and by the all but absolute perfection of its results. Indeed, the very first screw made by this process ranks to-day as the most perfect in the world. But such an engine as this might only be worked up to its highest efficiency under the most favorable physical conditions, and in its installation and use the most careful attention was given to the elimination of errors due to variation of temperature, earth tremors, and other disturbances. Not content, however, with perfecting the machinery by which gratings were ruled, Rowland proceeded to improve the form of the grating itself, making the capital discovery of the concave grating, by means of which a large part of the complex and otherwise troublesome optical accessories to the diffraction spectroscope might be dispensed with. Calling to his aid the wonderful skill of Brashear in making and polishing plane and concave surfaces, as well as the ingenuity and patience of Schneider, for so many years his intelligent and loyal assistant at the lathe and work-bench, he began the manufacture and distribution all too slowly for the anxious demands of the scientific world, of those beautifully simple instruments of precision which have contributed so much to the advance of physical science during the past twenty years. While willing and anxious to give the widest possible distribution to these gratings, thus giving everywhere a new impetus to optical research, Rowland meant that the principal spoils of the victory should be his, and to this end he constructed a diffraction spectrometer of extraordinary dimensions and began his classical researches on the solar spectrum. Finding photography to be the best means of reproducing the delicate spectral lines shown by the concave grating, he became at once an ardent student and shortly a master of that art. The outcome of this was that wonderful "Photographic Map of the Normal Solar Spectrum," prepared by the use of concave gratings six inches in diameter and twenty-one and a half feet radius, which is recognized as a standard everywhere in the world. As a natural supplement to this he directed an elaborate investigation of absolute wave-lengths, undertaking to give finally the wave-length of not only every line of the solar spectrum, but also of the bright lines of the principal elements, and a large part of this monumental task is already completed, mostly by Rowland's pupils and in his laboratory.

Time will not allow further expositions of the important consequences of his invention of the ruling engine and the concave grating.

Indeed, the limitations to which I must submit compel the omission of even brief mention of many interesting and valuable investigations relating to other subjects begun and finished during these years of activity in optical research, many of them by Rowland himself and many of them by his pupils, working out his suggestions and constantly stimulated by his enthusiasm. A list of titles of papers emanating from the physical laboratory of the Johns Hopkins University during this period would show somewhat of the great intellectual fertility which its director inspired, and would show, especially, his continued interest in magnetism and electricity, leading to his important investigations relating to electric units and to his appointment as one of the United States delegates at important international conventions for the better determination and definition of these units. In 1883 a committee appointed by the Electrical Congress of

1881, of which Rowland was a member, adopted 106 centimetres as the length of the mercury column equivalent to the absolute ohm, but this was done against his protest, for his own measurements showed that this was too small by about three-tenths of one per cent. His judgment was confirmed by the Chamber of Delegates of the International Congress of 1893, of which Rowland was himself president, and by which definite values were given to a system of international units.

Rowland's interest in applied science cannot be passed over, for it was constantly showing itself, often, perhaps, unbidden, an unconscious bursting forth of that strong engineering instinct which was born in him, to which he often referred in familiar discourse, and which would unquestionably have brought him great success and distinction had he allowed it to direct the course of his life. Although everywhere looked upon as one of the foremost exponents of pure science, his ability as an engineer received frequent recognition in his appointment as expert and counsel in some of the most important engineering operations in the latter part of the century. He was an inventor, and might easily have taken first rank as such had he chosen to devote himself to that sort of work. During the last few years of his life he was much occupied with the study of alternating electric currents and their application to a system of rapid telegraphy of his own invention. A year ago his system received the award of a *grand prix* at the Paris Exposition, and only a few weeks after his death the daily papers published cablegrams from Berlin announcing its complete success as tested between Berlin and Hamburg, and also the intention of the German postal department to make extensive use of it.

But behind Rowland, the profound scholar and original investigator, the engineer, mechanician, and inventor, was Rowland the man, and any estimate of his influence in promoting the interests of physical science during the last quarter of the nineteenth century would be quite inadequate if not made from that point of view. Born at Honesdale, Pennsylvania, on November 27, 1848, he had the misfortune, at the age of eleven years, to lose his father by death. This loss was made good, as far as it is possible to do so, by the loving care of mother and sisters during the years of his boyhood and youthful manhood. From his father he inherited his love for scientific study, which from the very first seems to have dominated all of his aspirations, directing and controlling most of his thoughts. His father, grandfather, and great-grandfather were all clergymen and graduates of Yale College. His father, who is described as one "interested in chemistry and natural philosophy, a lover of nature, and a successful trout fisherman," had felt, in his early youth, some of the desires and ambitions that afterward determined the career of his distinguished son, but yielding, no doubt, to the influence of family tradition and desire, he followed the lead of his ancestors. It is not unlikely, and it would not have been unreasonable, that similar hopes were entertained in regard to the future of young Henry, and his preparatory school work was arranged with this in view. Before being sent away from home, however, he had quite given himself up to chemical experiments, glass-blowing, and other similar occupations, and the members of his family were often summoned by the enthusiastic boy to listen to lectures which were fully illustrated by experiments, not always free from prospective danger. His spare change was invested in copper wire and the like, and his first five dollar bill brought him, to his infinite delight, a small galvanic battery. The sheets of the *New York Observer*, a treasured family newspaper, he converted into a huge hot-air balloon, which, to the astonishment of his family and friends, made a brilliant ascent and flight, coming to rest, at last, and in flames, on the roof of a neighboring house, and resulting in the calling out of the entire fire department of the town. When urged by his boy friends to hide himself from the rather threatening consequences of his first experiment in aeronautics, he courageously marched himself to the place where the balloon had fallen, saying, "No! I will go and see what damage I have done." When a little more than sixteen years old, in the spring of 1865, he was sent to Phillips Academy, at Andover, to be fitted for entering the academic course at Yale. His time there was given entirely to the study of Latin and Greek, and he was in every way out of harmony with his environment. He seems to have quickly and thoroughly appreciated this fact, and his very first letter from Andover is a cry for relief. "*Oh, take me home!*" is the boyish scrawl covering the last page of that letter, on another of which he says, "It is simply horrible; I can never get on here." It was not that he could not learn Latin and

Greek if he was so minded, but that he had long ago become wholly absorbed in the love of nature and in the study of nature's laws, and the whole situation was to his ambitious spirit most artificial and irksome. Time did not soften his feelings or lessen his desire to escape from such uncongenial surroundings, and, at his own request, Dr. Farrand, principal of the Academy at Newark, New Jersey, to which city the family had recently removed, was consulted as to what ought to be done. Fortunately for everybody, his advice was that the boy ought to be allowed to follow his bent, and, at his own suggestion, he was sent in the autumn of that year to the Rensselaer Polytechnic Institute at Troy, where he remained five years, and from which he was graduated as a civil engineer in 1870.

It is unnecessary to say that this change was joyfully welcomed by young Rowland. At Andover the only opportunity that had offered for the exercise of his skill as a mechanic was in the construction of a somewhat complicated device, by means of which he outwitted some of his schoolmates in an early attempt to haze him, and in this he took no little pride. At Troy he gave loose rein to his ardent desires, and his career in science may almost be said to begin with his entrance upon his work there and before he was seventeen years old.

He made immediate use of the opportunities afforded in Troy and its neighborhood for the examination of machinery and manufacturing processes, and one of his earliest letters to his friends contained a clear and detailed description of the operation of making railroad iron, the rolls, shears, saws, and other special machines being represented in uncommonly well executed pen drawings. One can easily see in this letter a full confirmation of a statement that he occasionally made later in life, namely, that he had never seen a machine, however complicated it might be, whose working he could not at once comprehend. In another letter, written within a few weeks of his arrival in Troy, he shows in a remarkable way his power of going to the root of things, which even at that early age was sufficiently in evidence to mark him for future distinction as a natural philosopher. On the river he saw two boats equipped with steam pumps, engaged in trying to raise a half-sunken canal-boat by pumping the water out of it. He described engines, pumps, etc., in much detail, and adds, "But there was one thing that I did not like about it; they had the end of their discharge pipe about ten feet above the water, so that they had to overcome a pressure of about five pounds to the square inch to raise the water so high, and yet they let it go after they got it there, whereas if they had attached a pipe to the end of the discharge pipe and let it hang down into the water, the pressure of water on that pipe would just have balanced the five pounds to the square inch in the other, so that they could have used larger pumps with the same engines and thus have got more water out in a given time."

The facilities for learning physics in his day at the Rensselaer Polytechnic Institute were none of the best, a fact which is made the subject of keen criticism in his home correspondence, but he made the most of whatever was available and created opportunity where it was lacking. The use of a turning lathe and a few tools being allowed, he spent all of his leisure in designing and constructing physical apparatus of various kinds with which he experimented continually. All of his spare money goes into this and he is always wishing he had more. While he pays without grumbling his share of the expense of a class supper, he cannot help declaring that "it is an *awful* price for one night's pleasure. Why, it would buy another galvanic battery." During these early years his pastime was the study of magnetism and electricity, and his lack of money for the purchase of insulated wire for electro-magnetic apparatus led him to the invention of a method of winding naked copper wire, which was later patented by some one else and made much of. Within six months of his entering the Institute he had made a delicate balance, a galvanometer, and an electrometer, besides a small induction coil and several minor pieces. A few weeks later he announces the finishing of a Ruhmkorff coil of considerable power, a source of much delight to him and to his friends. In December, 1866, he began the construction of a small but elaborately designed steam-engine, which ran perfectly when completed and furnished power for his experiments. A year later he is full of enthusiasm over an investigation which he wishes to undertake to explain the production of electricity when water comes in contact with red-hot iron, which he attributes to the decomposition of a part of the water. Along with all of this and much more he maintains a good standing in his regular work in the Institute, in some of which he is naturally the leader. He occasionally writes: "I am head of my class in mathematics," or "I lead the class in natural philosophy," but official records show that he was now and then "conditioned" in subjects in which he had no special interest. As early as 1868, before his twentieth birthday, he decided that he must devote his life to science. While not doubting his ability "to make an excellent engineer," as he declares, he decides against engineering, saying: "You know that from a child I have been extremely fond of experiment. This liking instead of decreasing has gradually grown upon me until it has become a part of my nature, and it would be folly for me to attempt to give it up; and I don't see any reason why I should wish it, unless it be avarice, for I never expect to be a rich man. I intend to devote myself hereafter to *science*. If she gives me wealth, I will receive it as coming from a friend, but if not, I will not murmur."

He realized that his opportunity for the pursuit of science was in becoming a teacher, but no opening in this direction presenting itself, he spent the first year after graduation in the field as a civil engineer. This was followed by a not very inspiring experience as instructor in natural science in a Western college, wl ere he acquired, however, experience and useful discipline.

In the spring of 1872 he returned to Troy as instructor in physics, on a salary the amount of which he made conditional on the purchase by the Institute of a certain number of hundreds of dollars' worth of physical apparatus. If they failed in this, as afterward happened, his pay was to be greater, and he strictly held them to the contract. His three years at Troy as instructor and assistant professor were busy, fruitful years. In addition to his regular work, he did an enormous amount of study, purchasing for that purpose the most recent and most advanced books on mathematics and physics. He built his electro-dynamometer and carried out his first great research. As already stated, this quickly brought him reputation in Europe and, what he prized quite as highly, the personal friendship of Maxwell, whose ardent admirer and champion he remained to the end of his life. In April, 1875, he wrote, "It will not be very long before my reputation reaches this country," and he hoped that this would bring him opportunity to devote more of his time and energy to original research.

This opportunity for which he so much longed was nearer at hand than he imagined. Among the members of the Visiting Board at the West Point Military Academy in June, 1875, was one to whom had come the splendid conception of what was to be at once a revelation and a revolution in methods of higher education. In selecting the first faculty for an institution of learning which, within a single decade, was to set the pace for real university work in America, and whose influence was to be felt in every school and college of the land before the end of the first quarter of a century, Dr. Gilman was guided by an instinct which more than all else insured the success of the new enterprise. A few words about Rowland from Professor Michie, of the Military Academy, led to his being called to West Point by telegraph, and on the banks of the Hudson these two walked and talked, "he telling me," Dr. Gilman has said, "his dreams for science, and I telling him my dreams for higher education." Rowland, with characteristic frankness, writes of this interview, "Professor Gilman was very much pleased with me," which, indeed, was the simple truth. The engagement was quickly made. Rowland was sent to Europe to study laboratories and purchase apparatus, and the rest is history, already told and everywhere known.

Rowland's personality was in many respects remarkable. Tall, erect, and lithe in figure, fond of athletic sports, there was upon his face a certain look of severity which was, in a way, an index of the exacting standard he set for himself and others. It did not conceal, however, what was, after all, his most striking characteristic, namely, a perfectly frank, open and simple straightforwardness in thought, in speech, and in action. His love of truth held him in supreme control, and, like Galileo, he had no patience with those who try to make things appear otherwise than as they actually are. His criticisms of the work of others were keen and merciless, and sometimes there remained a sting of which he himself had not the slightest suspicion. "I would not have done it for the world," he once said to me after being told that his pitiless criticism of a scientific paper had wounded the feelings of its author. As a matter of fact he was warm-hearted and generous, and his occasionally seeming otherwise was due to the complete separation, in his own mind, of the product and the personality of the author. He possessed that rare power, habit in his case, of seeing himself, not as others see him, but as he saw others. He looked at himself and his own work exactly as if he had been another person, and this gave rise to a frankness of expression regarding his own performance which sometimes impressed strangers unpleasantly, but which, to his friends, was one of his most charming qualities. Much of his success as an investigator was due to a firm confidence in his own powers, and in the unerring course of the logic of science which inspired him to cling tenaciously to an idea when once he had given it a place in his mind. At a meeting of the National Academy of Sciences in the early days of our knowledge of electric generators, he read a paper relating to the fundamental principles of the dynamo. A gentleman who had had large experience with the practical working of dynamos listened to the paper, and at the end said to the Academy that unfortunately practice directly contradicted Professor Rowland's theory, to which instantly replied Rowland, "So much the worse for the practice," which indeed, turned out to be the case.

Like all men of real genius, he had phenomenal capacity for concentration of thought and effort. Of this, one who was long and intimately associated with him remarks, "I can remember cases when he appeared as if drugged from mere inability to recall his mind from the pursuit of all-absorbing problems, and he had a triumphant joy in intellectual achievement such as we would look for in other men only from the gratification of an elemental passion." So completely consumed was he by fires of his own kindling that he often failed to give due attention to the work of others, and some of his public utterances give evidence of this curious neglect of the historic side of his subject.

As a teacher his position was quite unique. Unfit for the ordinary routine work of the class-room, he taught as more men ought to teach, by example rather than by precept. Says one of his most eminent pupils, "Even of the more advanced students only those who were able to brook severe and searching criticism reaped the full benefit of being under him; but he contributed that which, in a university, is above all teaching of routine, the spectacle of scientific work thoroughly done and the example of a lofty ideal."

Returning home about twenty years ago. after an expatriation of several years, and wishing to put myself in touch with the development of methods of instruction in physics, and especially in the equipment of physical laboratories, I visited Rowland very soon after, as it happened, the making of his first successful negative of the solar spectrum. That he was completely absorbed in his success was quite evident, but he also seemed anxious to give me such information as I sought. I questioned him as to the number of men who were to work in his laboratory, and although the college year had already begun he appeared to be unable to give even an approximate answer. "And what will you do with them?" I said. "Do with them?" he replied, raising the still dripping negative so as to get a better light through its delicate tracings, "Do with them? *I shall neglect them.*" The whole situation was intensely characteristic, revealing him as one to whom the work of a drill-master was impossible, but ready to lead those who would be led and could follow. To be neglected by Rowland was often indeed more stimulating and inspiring than the closest personal supervision of men lacking his genius and magnetic fervor.

In the fullness of his powers, recognized as America's greatest physicist, and one of a very small group of the world's most eminent, he died on April 16, 1901, from a disease the relentless progress of which he had realized for several years and opposed with a splendid but quiet courage.

It was Rowland's good fortune to receive recognition during his life in the bestowal of degrees by higher institutions of learning; in election to membership in nearly all scientific societies worthy of note in Europe and America; in being made the recipient of medals of honor awarded by these societies; and in the generously expressed words of his distinguished contemporaries. It will be many years, however, before full measure can be had of his influence in promoting the interests of physical science, for with his own brilliant career, sufficient of itself to excite our profound admiration, must be considered that of a host of other, younger men, who lighted their torches at his flame and who will reflect honor upon him whose loss they now mourn by passing on something of his unquenchable enthusiasm, something of his high regard for pure intellectuality, something of his love of truth and his sweetness of character and disposition.

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Theodore Lyman

1874

BIOGRAPHICAL MEMOIR

OF

THEODORE LYMAN.

1833-1897.

BY

H. P. BOWDITCH.

READ BEFORE THE NATIONAL ACADEMY OF SCIENCES,
APRIL 23, 1903.

BIOGRAPHICAL MEMOIR OF THEODORE LYMAN.

THEODORE LYMAN was born in Waltham, Mass., on the 23d of August, 1833, and died at Nahant, Mass., on the 9th of September, 1897. He was of the seventh generation in descent from Richard Lyman, the ancestor of the family, who came to this country in 1631 in the same ship with John Eliot, and the third successive bearer of the name Theodore Lyman.

The first Theodore Lyman, the grandfather of our late associate, came from old York, Maine, to Boston, and, as a successful merchant in Boston, laid the foundation of the family fortunes.

His son, the second Theodore Lyman, studied in Europe in his early life, and, returning, served in the State Legislature from 1820 to 1825. He was mayor of Boston in 1834–'35, and while in this office defended William Lloyd Garrison from personal violence at the hands of a mob of respectable rioters to whom the fearless course of the abolitionist leader had given grave offense. Mayor Lyman secured the foundation of the Massachusetts State Reform School at Westboro, now appropriately known as the Lyman School, in grateful recognition of his endowment of the institution with a fund amounting to $72,500. He was a generous friend to the Massachusetts Historical Society, and to the Boston Farm School, an institution over which his son presided for several years. He was the author of works upon "The Political State of Italy" and "The Diplomacy of the United States," of small volumes entitled "Rambles in Italy" and "A few Weeks in Paris during the Residence of the Allied Sovereigns in that City," and of a Fourth of July oration delivered in 1820.

The subject of the present sketch inherited his distinguished father's physique, as well as his intellectual traits and his strong sense of civic duties. He secured his early education from private instructors, spending two years in Europe from 1847 to 1849. While in Paris he suffered from a severe attack of typhoid fever, and also from weakness of the eyes. Returning home in 1849, he entered Harvard College in the class of 1855, having among his classmates Alexander Agassiz and Phillips Brooks. We find evidences of his literary activity during his college course in the pages of the *Harvard Magazine*, a periodical founded by the classes of 1855 and 1856, but destined to be short-lived. As if anticipating a career which was ten years later to engross his whole life and thought, his contributions were frequently upon military subjects, on which, as his classmate, F. B. Sanborn, says, "he joked with a substratum of excellent sense." His literary reputation as a student will, however, always rest securely on the song in which, as chorister of the Hasty Pudding Club, he described the mystical origin of that ancient fraternity.

After graduation he studied for three years under the guidance of Prof. Louis Agassiz, and in 1858 received the degree of S. B., *summa cum laude*. The impressions produced upon him at this period of his life are recorded in an article entitled "Recollections of Agassiz," published in the *Atlantic Monthly* in 1874. The direction given to his studies by his great master was maintained during his whole life, and in recognition of the high value of his biological work his Alma Mater bestowed upon him in 1891 the honorary degree of LL. D.

Theodore Lyman's first public service was rendered in 1859–'60 as a trustee of the Reform School, which had been founded by the state at the instance and with the help of his father, for the instruction, employment, and reformation of juvenile offenders unfit to be at large, but not for boys who had become hardened by a prolonged vicious course, who were bad themselves and fitted to make others bad. By degrees, however, this purpose had been lost sight of, and vicious youths up to sixteen years of age had been committed to the school. The natural consequences ensued: $50,000 worth of property was destroyed by the burning of newly erected buildings by a boy who thus attempted to secure an alternate sentence—*i. e.*, a short sentence to a penal institution, instead of being kept under guardianship at the school during minority. A return to the original plan of the founders of the school was secured through the strenuous exertions of Theodore Lyman, who, though the youngest member of the board, evidently prevailed in their counsels through the same effective courage and energy which marked his later career, and did not quit the work until the Legislature had fixed the age limit at fourteen years, and had done away with the alternate sentence, placing all the boys in the school's custody during minority. It was not until 1884, when the Massachusetts Reformatory was established at Concord, that the age limit at Westboro was fixed at fifteen years, and provision was made for the transfer to Concord of boys who should prove to be unfit subjects for the Reform School, which was now, by act of Legislature, called "The Lyman School for Boys." A few years after its removal to a neighboring farm in the town of Westboro, Theodore Lyman came to the school for the dedication of the chapel, and, as he watched the boys at their work and play, he expressed his satisfaction at the success of the trustees in having at last made it very nearly the kind of school that his father had wished and hoped that it might become.

Theodore Lyman was married on November 28, 1856, to Elizabeth Russell, eldest daughter of George R. Russell, and a few years later went abroad for two years. It was during this period that his daughter Cora was born, in 1862. Returning home in 1863, he at once entered the military service of his country, then in the throes of civil war. He secured a commission as volunteer aid of the Governor of Massachusetts with the rank of lieutenant colonel, and was assigned to special duty at the headquarters of General Meade, with whom he had become very well acquainted before the war, and who was then in command of the Army of the Potomac. In this capacity he served till the end of the civil war, taking part in the battles of the Wilderness, Spottsylvania Court House, and Cold Harbor, in the movements around Petersburg, and in the final surrender at Appomattox Court House, where he was one of the few officers privileged to ride through the Confederate lines after the surrender. During all this period he showed an active and intelligent interest in his new work by making almost daily sketches showing the positions of the different corps of the Army of the Potomac. Mr. John C. Ropes, president of the Military Historical Society of Massachusetts, writes that he "was so much impressed with the value of these cartographic statements of the movements of the Army of the Potomac, from the autumn of 1863 down to and including the 9th of April, 1865, when Lee surrendered," that he had them all copied for the use of the Society. The same high authority in military matters speaks also of having seen extracts from a diary kept by Theodore Lyman during this period, "which are as humorous and as entertaining as any pictures of the camp and march can possibly be." It is greatly to be hoped that this diary may in due time be edited and published, as it cannot fail to be a valuable contribution to our knowledge of the civil war. Few actors in this great drama had better opportunities of watching the succession of important historical events, or minds better qualified for observing, recording, and commenting upon them. Nor did his interest in military matters cease with the war, for, as a member of the Military Historical Society of Massachusetts he had ample opportunity to discuss with his companions in arms the great events in which they had all taken part. On June 11, 1877, he read a "Review of the Reports of Colonel Haven and General Weld on the conduct of General McClellan at Alexandria, in August, 1862, and on the case of Fitz John Porter."

Lyman maintained a close and unbroken friendship with General Meade until the death of the latter, in 1872. He then wrote an obituary notice of his old commander, which was published in volume IX of the Proceedings of the American Academy of Arts and Sciences.

During the twenty years following the close of the civil war Theodore Lyman's life was one of abounding activity, though before the end of this period the dread disease which was to make the closing years of his life a continual martyrdom had already marked him for its own.

To his Alma Mater he rendered important services as overseer from 1868 to 1880 and from 1881 to 1888. Here his influence was always thrown in favor of liberty in the choice of studies and in attendance upon religious services. He was also one of the original trustees and treasurer of the Zoölogical Museum, a member and secretary of the museum faculty, and assistant in zoölogy. The value of his services to the museum in these various capacities was gratefully acknowledged by the director, Alexander Agassiz, who, in his Annual Report for 1896–1897, thus speaks of Lyman's scientific work: "His zoölogical work began with short papers on ornithological subjects. He subsequently became interested in corals, and finally devoted himself specially to Ophiurans. The first illustrated catalogue of the museum was from his pen, and this important monograph on Ophiurans was followed by numerous papers on the same subject, treating of new species of the group. He wrote the Report on the Ophiurans of the *Hassler* Expedition, of the *Challenger*, and of the *Blake*, which include by far the larger number of species of Ophiurans dredged by those deep-sea exploring expeditions."

On the establishment of the Commission of Inland Fisheries, in 1866, Theodore Lyman became its first chairman, and gave the state devoted service for seventeen years without compensation. The story of his disinterested labor in this field is told in the commissioners' annual reports, many of which are from his own pen, and are characterized by a brightness of style which pleasantly relieves the gravity of an official document.

In 1884, as president of the American Fish Cultural Association, at the thirteenth annual meeting held in Washington on May 13, he delivered an address which is printed in the Nineteenth Annual Report of the Commissioners of Inland Fisheries of Massachusetts. Here he sketches in the most charming manner the history of the fish industries of New England from the time when the inhabitants were wont to "dunge their grounds with codd." He shows that fifty years after the settlement of the country a diminution in the number of fish in the New England rivers had already been noted, and describes the various laws enacted for their protection, culminating in 1864–'65 in modern fish culture under the auspices of several state governments, and finally in the appointment in 1871 of the United States Fish Commission under the leadership of Professor Spencer F. Baird.

The various fishery commissions of the country have, to use Theodore Lyman's own words, "accumulated a vast amount of accurate information concerning the numbers and variety of our fishes, their food, manner of breeding, condition of life, migration, and stages of growth." Pisciculture has become a state and national industry, while many private fish preserves have been established in various parts of the country. Several species of Salmonidæ are raised regularly for the market, and it is highly probable that nearly all the shad now taken in our Atlantic streams have originated in state or national hatching establishments. These results, though important, merely serve to indicate what great additions to the wealth of the country may be effected when water culture is "practised as universally and methodically as is agriculture." When Americans shall have learned to cultivate the water thus methodically, and shall desire to honor the men who in their day and generation have labored to re-establish the fisheries of the country, no name will stand higher on the list than that of Theodore Lyman.

In politics Theodore Lyman was distinguished for independence and an earnest advocacy of civil-service reform, a cause which, as founder and vice-president of the Massachusetts Reform Club, he sought in every way to promote. He was elected to Congress from the ninth district of Massachusetts in 1882, and, though handicapped by increasing infirmities, nobly represented the state "as long as patriotism was more prized in his district than partisanship."* His independent course in politics was naturally distasteful to many political leaders, and, at the time of the "Mugwump" defection from the Republican party, called down upon him some severe animadversions from Senator Hoar. On this occasion he, with exquisite humor and with generous consideration for his antagonist, compared himself to a fellow who boasted to his neighbors that he had "just been cuffed by the King."

In November, 1859, he was elected a resident fellow of the American Academy of Arts and Sciences and served as treasurer of that body from 1877 to 1883. He was also secretary of the committee on the one hundredth anniversary of the founding of the Academy.

In November, 1869, he was elected a resident member of the Massachusetts Historical Society, and in December, 1880, he read before the Society a memoir of his father-in-law, George R. Russell.

He was also a trustee of the Peabody Educational Fund and an honorary member of the New York Academy of Sciences.

He was elected a member of the National Academy of Sciences in 1872, and was transferred to the list of honorary members November 10, 1886. He was a member of the committees on the astronomical day, eclipse of 1886, and new observatory.

Theodore Lyman did so much work of a high order in so many different directions that it is difficult to decide in what calling he was most fitted to excel. That he possessed a decided aptitude for the duties of a soldier is the opinion of those best qualified to judge, and it is not at all improbable that when, in the

* "H. L.," Transcript, Sept. 15, 1897.

fullness of time, his diary shall be given to the public, his place among the military writers of the world will be definitely assured.

His high character and his firm conviction that "public office is a public trust" well fitted him for the career of a statesman, and there is little doubt that he would have distinguished himself in public life had circumstances favored the adoption of such a career.

His scientific papers are examples of conscientious observation, and are valuable contributions to the field of knowledge in which he labored.

Perhaps the trait of character which most impressed itself upon all who came in contact with Theodore Lyman was the cheerfulness and gayety of his disposition. This gayety was far removed from frivolity, and was compatible with a stern expression of indignation whenever circumstances called it forth. In this spirit were compiled the "Papers relating to the Garrison Mob,"* in which the son indignantly repels the criticisms of Wendell Phillips upon the conduct of the father on that memorable occasion.

Another remarkable trait in Theodore Lyman's character was the alertness of his mind, which, combined with the gayety of his nature, made his companionship both socially and intellectually so charming. Even in his serious writings, *e. g.*, in his reports as Fish Commissioner, his exuberant vitality and his cheerful humor found expression, but it was, of course, in his personal intercourse with his companions that this charm was most distinctly felt. It was this which led to his being for many years, by common consent, chairman of his class dinners, as the class secretary, E. H. Abbot, tells us in a most appropriate and affectionate notice of him prepared for distribution to the members of the class of 1855.

The members of the Thursday Evening Club, over which he presided for many years, will long remember the way in which the meetings were enlivened by his ready wit, and the happy manner in which he introduced the successive speakers.

Upon this life, so filled with everything that could make life enjoyable, early fell the shadow of a mortal disease, so gradual

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*Cambridge: Welch, Bigelow & Co., 1870.

in its approach that few of his friends were aware that the first warning came sixteen years before his death. During this period he was, in the words of his friend and classmate, E. H. Abbot, "day by day parting with the power to act, until at last he was forced to stand still and watch the stream of life flow by; a soul imprisoned in a body which was gradually losing all power of movement, and which at last became absolutely helpless and dependent for every service upon external aid. To him of all men these years of prolonged and growing uselessness must have seemed a living death. And yet they who know most about him know that those years were really the noblest of his life. His brave spirit in this growing isolation, which at last withdrew him from the sight of almost all except his own family, surmounted all barriers. He never permitted himself to lose his active interest in the events of other lives. He cheered on the doers of good all over the world by messages which came from his chamber with all their old-time gayety and brightness. When his hand could no longer hold the pen, he spoke through his tender amanuensis words full of the same high courage and cheerful humor which had been his charm in earlier life."

In concluding this brief notice of Theodore Lyman, it may perhaps be permitted to supplement the above tender and truthful description of his last years with a few words employed by the writer of this sketch to introduce a resolution of the Thursday Evening Club replying to an affectionate message from its former president on the fiftieth anniversary of its foundation:

"I remember, Mr. President, when a young man, looking around among the men of my generation and considering whose lot in life seemed to me, on the whole, the most enviable. I came to the conclusion that Theodore Lyman was, of all my acquaintances, the man for whom the future seemed to hold out the brightest promise.

"In vigorous health, with a personality—physical, mental, and moral—which endeared him to all who came in contact with him, happily married, with instincts and powers which led him to the highest callings, to the service of his country in the field and in legislative halls, with tastes for the study of the natural sciences and abundant means to gratify them, there seemed to be nothing lacking to make his life an ideally happy one.

"Then, when the shadow of a slow, insidious disease fell upon him, it seemed for a time as if his life were but to afford another illustration of the old Greek saying that no man is to be judged happy before his death; but when I saw how bravely he met the advances of his enemy, and with what courageous cheerfulness he interested himself in the pursuits of his friends and in the active life around him in which he could no longer share, I could not help feeling that a happiness was reserved for him higher than any of which the Greek philosopher had dreamed or I, as a young man, had formed a conception—the happiness of knowing that by the force of his example he had helped to raise those who came under its influence to a higher and nobler life."

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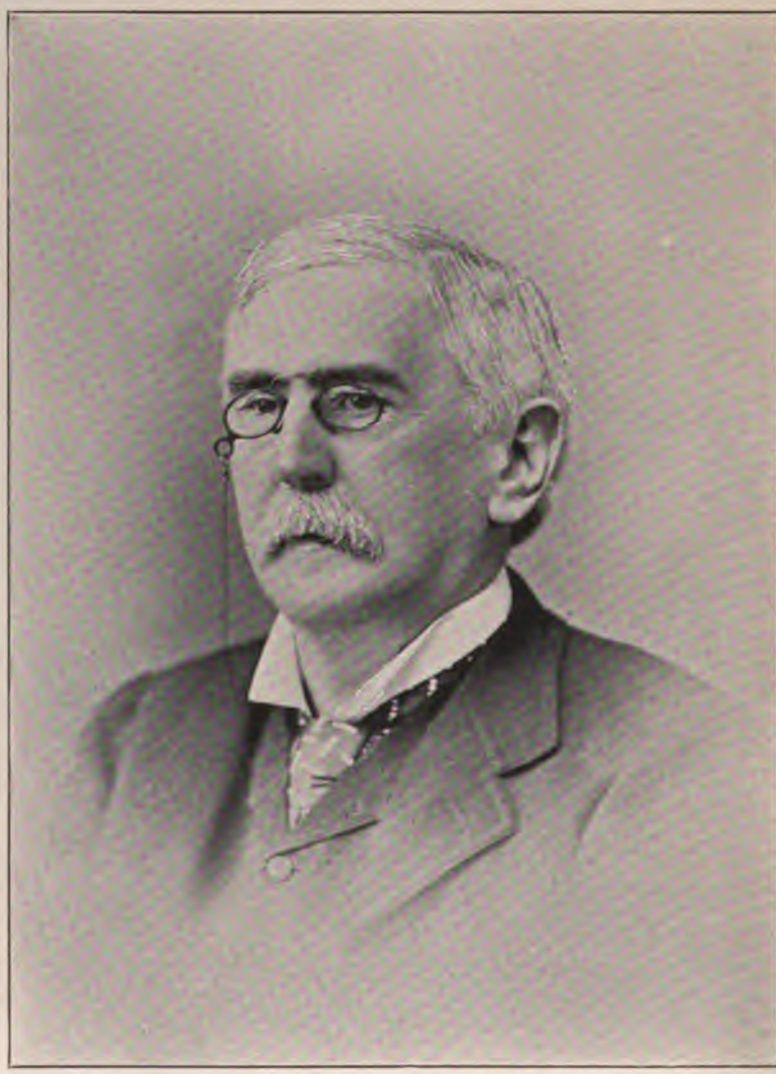

M. Carey Lea.

BIOGRAPHICAL MEMOIR

OF

MATTHEW CAREY LEA.

1823-1897.

BY

GEORGE F. BARKER.

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BIOGRAPHICAL MEMOIR OF MATTHEW CAREY LEA.

Perhaps there is no department of physical science whose seductions are more alluring or whose results are more fascinating than that of chemistry. The nature of matter has always been, even from the earliest times, one of the most favorite speculations of the human mind; and the study of the relations of the molecule to the atom, and therefore of the part which these minute portions of matter play in natural phenomena, has ever been one of the most interesting subjects for research. In no department of these so-called matter-sciences, however, has more original and suggestive work been done probably than in that of photographic chemistry, based as it is so largely upon the nature of the metal silver; and no investigator apparently has added to our knowledge more valuable facts in this direction than our late worthy colleague who is the subject of the present memoir.

MATTHEW CAREY LEA was born in Philadelphia August 18, 1823. His father, Isaac Lea, the distinguished naturalist, belonged to one of the old Quaker families of that city, his grandfather, John Lea, who had come to America in 1700 with William Penn, having been eminent for many years as a preacher at the Friends' meetings. His mother was Frances A. Carey, the daughter of Matthew Carey, the eminent and prolific writer on political economy and a publisher of repute. Upon his marriage, in 1821, Isaac Lea became a member of the publishing firm of M. Carey & Sons, in which he continued as an active partner for more than thirty years. He early became interested in science, devoting himself to natural history, and especially to conchology. His "Observations on the Genus Unio," 1827–1874, fill thirteen quarto volumes, containing two hundred and eighty plates. His investigations extended also to geology, mineralogy, and paleontology. He became a member of the Academy of Natural Sciences in Philadelphia in 1815, and was chosen its President in 1858. In 1860 he was elected president of the American Association for the Advancement of Science. He died in Philadelphia in 1886, at the age of 95 years, bequeathing his unrivaled collection of shells and minerals to the United States National Museum.

Carey Lea was the second son in a family of three sons and a daughter, the eldest son dying in infancy. His younger brother, Mr. Henry C. Lea, the well-known publisher and a writer of great eminence on philosophical and historical subjects, is still living in Philadelphia.

Trained amidst such scientific surroundings, it is not surprising that our late fellow-member early developed a decided taste in this direction. But as his constitution was never a vigorous one, he was sent neither to school nor to college, but spent the early part of his life at his home, where he was given a very broad and thorough education by the best private tutors procurable. In this way his strong intellectual powers and retentive memory enabled him to acquire a culture of great value, especially in languages, in literature, and in the natural and physical sciences. His father's scientific eminence, coupled with the possession of ample means, greatly facilitated the development of his powers. In 1832, at the age of 9 years, he spent six months in Europe with his parents, where he was brought into contact with eminent men of science, both in England and on the Continent, many of whom had long been friends of his father.

At first his choice was for the legal profession, and he entered the office of the Hon. Wm. M. Meredith, then the leader of the Philadelphia bar. After the completion of his studies he was admitted to practice about the year 1847. Continued ill health, however, the result of overstudy, forced him not long afterward to abandon the profession of the law. He went again to Europe, where he spent some years in hope of permanent relief, but without success. From that time he remained more or less an invalid, having repeated attacks of illness, which largely incapacitated him for the more active pursuits of life.

His great interest in scientific matters, and especially in chemistry, led him, not long after his return from Europe, to enter the laboratory of Professor James C. Booth, where he acquired that proficiency in chemical science and that love of research which distinguished him ever afterward. His later experimental work was mainly done in the private laboratory at his home in Chestnut Hill. His more important papers, a list of the titles of which is appended to this memoir, number about one hundred, the most of which appeared in the *American Journal of Science* during the years from 1860 to 1897. But his minor papers, especially those on photography, published in the technical journals, far exceeded this number. Those in the *British Journal of Photography* alone number nearly three hundred.

Matthew Carey Lea's earliest scientific paper was entitled "On the First or Southern Coal Field of Pennsylvania." It appeared in 1841 in the *American Journal of Science and Arts*, and contains the results of his proximate analysis of fifteen authentic samples of Pennsylvania coal, these analyses having been made in Professor Booth's laboratory. The easternmost coal examined was from the Lehigh region. It contained 87 per cent. of carbon, 7.30 per cent. of volatile matter, and 5.70 per cent. of ash. The westernmost came from Rattling Run Gap, and consisted of carbon, 76.10 per cent., of volatile matter, 16.90 per cent., and of ash, 7 per cent. The highest percentage of fixed carbon in these coals was contained in that from Tamaqua, 91 per cent. The lowest was in that from Yellow Springs Gap, 74.70 per cent. These analyses were made with the object of testing the truth of an opinion expressed by his father, that "the hard or highly carbonized anthracite of the eastern end of the southern coal field changes to the bituminous in the western end by nearly regular gradations." "It would appear from these results," he says, "that the bituminous qualities of the coal increase with considerable regularity from Tamaqua to Rattling Run."

The purely chemical investigations of Carey Lea, both in organic as well as in inorganic chemistry, covered a wide range of subjects. Like many others, he indulged in speculations on chemical theory, especially upon the connection between the properties of the atoms and their numerical relations. In the first part of his paper "On Numerical Relations existing between the Equivalent Numbers of Elementary Bodies," he undertook to show "that the number 44.45 plays an important part in the science of stoichiometry, and that the relations which depend upon it are supported, in some cases at least, in a remarkable manner, by analogies of atomic volume." Since solids and liquids are very far from being governed in their volume combinations by the simple laws which control gases, we may expect, if occasionally we find such a coincidence, to observe a close relationship between the substances thus united. But more than this: If, for example, we find that a given volume of silver unites with a given volume of oxygen, and that the same volume of gold unites with precisely the same volume of oxygen, we may "conjecture that gold may differ from silver only by a third substance which unites with the silver without increasing its volume or affecting the amount of oxygen which it is capable of saturating, but which, on the other hand, alters its chemical equivalent, its specific gravity and other physical characters." Further, "if we find that by subtracting from the chemical equivalent of silver half the difference between the equivalents of silver and gold, we obtain the equivalent of a third metal, copper (Cu=63.4), which also, under equal volumes, combines with a quantity of oxygen expressed by a very simple relation with that capable of saturating gold and silver, we may at least speculate that the three may form a series consisting of two substances combined in different proportions." His numerical computations show that the relation spoken of extends to no less than forty-eight of the elementary bodies.

In the second part of this paper a new and wholly distinct relation is pointed out, which the author calls a relation of "geometrical ratios" to distinguish it from the relation of arithmetical differences discussed in the first paper. Their nature consists in this: "that if we take two substances and examine the ratio which subsists between the numbers representing their atomic weights, we may find in certain cases that it is identical with the ratio subsisting between the atomic weights of two other substances." Thus the ratio between the atomic weights of oxygen and nitrogen, for example, is that of four to seven, which is exactly the same as that between those of zirconium and potassium and those of potassium and barium.

In July, 1864, Mr. Lea published two papers on the Platinum group, the first being entitled "Notes on the Platinum Metals and their separation from each other," and the second "On Reactions of the Platinum Metals." The material used was California osmiridium, and was obtained from Professor Booth, of the United States Mint. By following substantially the process of Claus, three portions were obtained: (*A*) a sandy crystalline precipitate of iridium sal-ammoniac, (*B*) a liquid obtained by washing this precipitate with saturated solution of sal-ammoniac, which carried through nearly the whole of the ruthenium as bichloride, and (*C*) another liquid obtained by using dilute solution of sal-ammoniac in the washing, and which contained, besides some ruthenium bichloride, small quantities of iridium and rhodium. In treating the portion (*A*) it was placed in a large flask, with twenty or twenty-five times its weight of water, and heated to boiling. Then crystals of oxalic acid were added; whereupon effervescence took place, and the iridium salt was rapidly reduced. On standing for a few days, after adding sal-ammoniac crystals to the boiling solution, the platinum sal-ammoniac separated as a crystalline powder. The mother liquor was then boiled with aqua regia, and, on cooling, the iridium sal-ammoniac crystallized out pure. This use of oxalic acid for purifying the double chloride of iridium and ammonium is here proposed for the first time and possesses marked advantages over the older methods. Its reducing action upon iridium bichloride at the boiling point is immediate, while it has no effect on the bichloride of ruthenium, and leaves the platinum in the condition of double chloride, remaining behind as a reddish powder. The paper discusses the reactions of caustic baryta upon iridium, and points out the marked difference between the action of this substance and that of the alkalies upon the platinum metals. It also calls attention to a new reaction for ruthenium, first observed by the author. When a solution of hyposulphite of soda is mixed with ammonia and a few drops of sesquichloride of ruthenium solution is added, a magnificent red-purple liquid is produced, which, unless quite dilute, is black by transmitted light. On testing the sensibility of this reagent it was found that $\frac{1}{5000}$ of the sesquichloride gave a bright rose purple, $\frac{1}{20000}$ to $\frac{1}{30000}$ a fine rose color, $\frac{1}{50000}$ a paler but still perfectly distinct color, and $\frac{1}{100000}$ a still paler but unmistakable rose tint. The chief value of this new test for ruthenium, the author says, lies in the fact that it is capable of detecting ruthenium in presence of an excess of iridium. No precautions are necessary, and the reaction is always obtained with the greatest facility.

In the second part of this paper Mr. Lea describes the reactions of hyposulphite of soda with the other platinum metals, and also gives the effect of the presence of these metals upon the ruthenium reaction. Even when iridium is present, the hyposulphite test is at least ten times more delicate than any other. Platinum in small quantities is without effect, and in larger quantities only changes the color from a rose to a wine red. Evidently this test is a valuable one for the purity of iridium. If the suspected iridium salt be boiled with hydrochloric acid, and ammonia added until the liquid assumes a pale olive color, then, on adding the hyposulphite and boiling, any increase of color indicates the presence of impurity. If the liquid acquires a rose color, ruthenium is present; if a wine color, platinum is probably present; and if brown, palladium is indicated. The paper concludes with a discussion of the reactions of the platinum metals with tetrathionic acid, with sulphate of quinia, with protochloride of tin, with ammonio-chloride of zinc, with ferrocyanide of potassium in caustic soda, and with Schlippe's salt. For ruthenium sesquichloride, the reaction with hyposulphite is characteristic; for the bichloride, that with ammonio-chloride of zinc, giving a rose-colored precipitate, is distinctive; for iridium the best test is the protochloride of tin and potash, and for palladium the reaction with tetrathionic acid is highly characteristic.

Conversely, the author has suggested ruthenium sesquichloride as a reagent for the detection of sodium hyposulphite. He has shown that a solution containing one four-thousandth of hyposulphite, when boiled with one of the ruthenium compounds made alkaline with ammonia, gives a clear rose-red color; one twelve-thousandth gives a well-marked pink liquid, and one containing one twenty-five-thousandth gives a salmon color.

In 1866 Mr. Lea called attention to the great increase of delicacy which is produced in the starch reaction for iodine by adding chromic acid to the solution. After adding the starch to the suspected liquid, add a drop or two of a dilute bichromate solution and then a few drops of hydrochloric acid. With solutions of iodide of potassium of $\frac{1}{1000000}$ the blue precipitate is abundant, and indications are observable at $\frac{1}{4000000}$, though at $\frac{1}{8000000}$ the result is somewhat doubtful.

Carey Lea described, in 1874, a new compound formed by the union of mercuric iodide with silver chloride, analogous to the double iodide of mercury and silver, and that of mercury and copper, which had already been obtained by Meusel. By adding to a weighed quantity of potassic iodide in solution an exactly equivalent quantity of mercuric chloride, agitating and allowing the precipitate to settle, and then adding an equivalent quantity of silver nitrate solution, three precipitates appear in the liquid—scarlet mercuric iodide, white silver chloride, and the yellow body which results from their union. After thorough admixture and after standing for twenty-four hours, the new compound appears as a heavy yellow powder, wholly free from any trace of red and inclining to a greenish or lemon-yellow color. Mixed with gum and spread on card-board, it dries to a full chrome yellow. The substance exhibits remarkable properties in its relations to heat. Even below 100° F. it begins to redden, and the color increases up to about 140°, when it has a bright scarlet color, resembling vermilion. On cooling, its natural color returns. The author does not agree with Meusel in the opinion that these substances are mechanical mixtures. He believes them to be true chemical compounds, though perhaps rather loose ones.

A paper by Carey Lea, "On the Nature of Certain Solutions and on a New Means of Investigating Them," was presented to the National Academy of Sciences at its April meeting in 1893. In this paper he pointed out that two classes of salts are formed by the three best known acids; the one perfectly neutral, like the alkali salts, the other decomposed by water like mercuric sulphate, bismuth nitrate, and stannous chloride. In the case of the sulphates, the number of metallic salts which have a neutral reaction is comparatively small. The question now is as to the meaning of the acid reaction of sulphates. Is free acid present in any or all of the cases? The unreliability of the ordinary tests for free sulphuric acid led the author to devise a new reaction by means of which the condition of such solutions may be examined and free sulphuric acid be detected in the presence of sulphates with great accuracy and sharpness, even when only a trace is present. The new method is based on the fact that from the sulphate of iodo-quinine, the well-known polarizing salt discovered by Herapath, it is possible to remove the whole of the sulphuric acid without breaking up the molecule. This may be done with either the carbonate or the hydroxide of barium, by placing this in a beaker, covering it with about 70 per cent. alcohol, and dropping into it the crystallized herapathite. In presence of the barium compound it dissolves readily and freely, forming a deep sherry-wine colored liquid, which, when allowed to dry spontaneously, leaves an amber-colored varnish without a trace of crystallization. If, however, a minute quantity of sulphuric acid be added to the solution, there is left behind, on evaporating it, a characteristic bluish black film with isolated crystals of iodo-quinine sulphate. When barium carbonate is used in the preparation—and it is preferable—some evolution of carbon dioxide occurs; whence it seems probable that the resulting solution contains a free base. From this solution the sulphate is readily regenerated by free, but not by combined, sulphuric acid. Even neutral sulphates of weak bases, like quinine and brucine, are wholly without action. From an extended investigation of the conditions, the author concludes: (1) that the solution of iodo-quinine affords the means of detecting free sulphuric acid even in traces, in presence of combined sulphuric acid; (2) that the salts of protoxides of the heavy metals do not owe their acid reaction to dissociation. With one exception, the solutions of their sulphates contain no free sulphuric acid. This exception is ferrous sulphate, whose solutions always contain free acid. (3) That sesquisulphates are always dissociated in solution; (4) that alums, with the exception of chrome alum, are always dissociated in solution; and (5) that acid salts are dissociated in solution; sometimes perhaps completely.

The following year another noteworthy paper by Carey Lea was communicated to the National Academy, entitled a "New Method of Determining the Relative Affinities of Certain Acids." This method was based on the principle that "the affinity of any acid is proportionate to the amount of base which it can retain in the presence of a strong acid selected as a standard of comparison for all acids." Suppose, for example, that sulphuric acid is taken as the standard, the quantity employed being always a gram-molecule at a fixed rate of dilution. Evidently two gram-molecules of sodium hydroxide would exactly saturate it. Taking, now, any given acid, it may be that the quantity of its sodium salt corresponding to three gram-molecules of sodium hydroxide exactly extinguishes the reaction of one gram-molecule of free sulphuric acid, the presence of the standard acid in the free state being ascertained by the herapathite test already mentioned. Suppose, now, that with a second acid we find that a quantity of its sodium salt corresponding to four gram-molecules of the hydroxide is needed to extinguish the sulphuric reaction. Then it follows that the affinity of the second acid is exactly twice as great as that of the first. At the point where the free sulphuric acid reaction was extinguished, the second acid retained twice as much sodium as the first, and this quite independently of any question of comparative basicity. Thus in the case of hydrochloric acid there was needed as a mean 29.37 gram-molecules of sodium chloride to extinguish the reaction in one gram-molecule of sulphuric acid. At this point the solution necessarily contained

$$Na_2SO_4 + (HCl)_2 + 27.37\ NaCl,$$

as is proved by the fact that the solution gives no longer a trace of free sulphuric acid reaction. Hence 27.37 gram-molecules of sodium chloride is the proportion of undecomposed sodium chloride that must remain in the solution in order that the sulphuric acid may be completely converted into sodium sulphate, and may remain as such in the solution in a condition of equilibrium. The number 27.37 therefore represents the strength of the affinity of hydrochloric acid for sodium. To compare acids of different basicities, however, it is convenient to refer them all to dibasic sulphuric acid, and therefore to divide this number by two; so that 13.68 may be taken as the index of the affinity of hydrochloric acid.

For pyrophosphoric acid, the mean value found for the quantity necessary to extinguish the free sulphuric acid in one gram-molecule of sulphuric acid was found to be 0.963 gram-molecule of sodium pyro phosphate. Hence at this point the liquid contains

$$Na_2SO_4 + \tfrac{1}{2}\,(H_4P_2O_7) + .463\ (Na_4P_2O_7)$$

in equilibrium. The number .463 therefore represents the comparative affinity of pyrophosphoric acid, except that as this acid is tetrabasic, it must be multiplied by two in order to bring it into comparison with dibasic acids. The index of pyrophosphoric acid is therefore .926; so that half a molecule contains the quantity of sodium required to saturate a molecule of sulphuric acid, and therefore only half a molecule of pyrophosphoric acid is set free. In other words, it is found by experiment that the quantity of pyrophosphate necessary to extinguish the reaction for free sulphuric acid with 1,000 molecules of that acid is 963 molecules. Out of this 500 molecules of pyrophosphoric acid are set free and there remain 463 molecules of undecomposed pyrophosphate. This number 463 multiplied by 2 because of the basicity of the acid, and divided by 1,000 to make it correspond to one molecule of sulphuric acid, gives .926 as the index of pyrophosphoric acid. The author points out that the state of equilibrium is always conditioned by the degree of concentration; so that if to any solution of sulphuric acid a salt is added in just sufficient quantity to extinguish the sulphuric-acid reaction, the addition of a little water at once changes the equilibrium. A certain portion of the salt added is reformed, and the sulphuric reaction reappears. By means of this method, therefore, it may be shown (1) that when to free sulphuric acid a salt is added in sufficient quantity to cause the whole of the sulphuric acid to saturate itself with the salt-base, it is possible by means of the herapathite test to determine the exact point of such saturation; (2) that a series of equilibria thus obtained with different salts enables us to determine the comparative strength of the affinities of the acids of those salts; (3) that the fact, already proved in other ways, that even small quantities of weak acids added to sulphates will set free a certain quantity of sulphuric acid can, by the means here given, be rendered for the first time visible to the eye by a well-marked chemical reaction.

The same year he proposed two new methods for reducing platinic to platinous chloride—one by the action of potassium acid sulphite, the other by that of alkali hypophosphites. In using the latter method, if the heat is continued after the conversion into the red salt is complete, the solution changes to dark brown, and has properties which seem to justify the assumption that it contains a platinum subchloride. Since silver chloride, after being acted on by light, yields nothing to nitric acid, it cannot have been reduced to metal. The tendency to form subchlorides in the case of both metals seems, therefore, unmistakable.

In Organic Chemistry, as well as in Inorganic, Carey Lea made many valuable investigations. He published very early a series of papers on picric acid and its compounds, giving in the first of these, which appeared in 1858, an important modification of one of the processes for preparing the acid. The yellow Australian gum from the *Xanthorrhea hastilis* is the most advantageous substance from which to prepare picric acid, from which it is obtained by acting on it with six or eight times its weight of strong nitric acid in the cold. In the new process the acid is added at intervals, and the action is moderated by the addition of water, the heat being carefully regulated. In this way the amount of nitric acid required is diminished one-half, and the violent evolution of fumes is avoided. In this paper are described the picrates of barium, glucinum, aluminum, manganese (manganous), iron (ferrous and ferric), cobalt, nickel, chromium (chromous and chromic), zinc, cadmium, copper, mercury (mercuric), silver, urea, and quinine.

In a second paper, read at the Newport meeting of the American Association, he discusses the claim made for picric acid as a test for potash, and shows that an alcoholic solution of picric acid or an aqueous solution of picrate of soda will produce a precipitate in almost any alkali solution, whether of soda, ammonia, or potash, except under circumstances of great dilution. Hence he infers that picric acid is wholly unreliable as a test for potash; in fact, that it is a better test for soda, because with soda solution it yields a precipitate which, when redissolved by heat, gives a characteristic spherically radiated bright canary yellow crystalline precipitate, whereas the precipitate given in a potash solution can never be positively distinguished from that thrown down in a solution of ammonia, the precipitate appearing when only half of one per cent. of the ammonia salt is present.

About the same time Carey Lea published a paper giving the results of his study of the ammonio-picrates, describing the salts of silver, copper, cobalt, zinc, cadmium, chromium, manganese and iron. In a preliminary paper he had shown that when certain metallic salts are precipitated by ammonia in excess, a more or less complete solution of the precipitate takes place. When treated with an alkali picrate, however, the solution yields an immediate precipitate of a compound of the base with picric acid and ammonia. These precipitates are often very beautiful, are generally yellow in color, and are nearly insoluble in water, which readily decomposes them, especially if added in excess.

In 1861 he gave a résumé of his observations on picric acid, considering its solubility in sulphuric acid, the tests for it, the methods of its purification, and the effect of reducing agents upon it. The most delicate tests for picric acid are: 1st, ammonio-sulphate of copper, which gives a greenish crystalline precipitate; 2d, potassium sulphide with excess of alkali, which on heating gives a deep red liquid; 3d, potassium cyanide, which also gives a red liquid on heating. A solution of $\frac{1}{10000}$ part of picric acid in water gives no precipitate with the first test, but responds to the others, the cyanide test being the more delicate of the two.

Mr. Lea subsequently made a number of studies on the ethyl and methyl bases. For preparing ethylamine he advises to mix nitrate of ethyl with its own volume of strong alcohol, and to add an equal bulk of concentrated solution of ammonia. On placing the liquid in strong sealed tubes and heating on the water bath to boiling for three hours, the reaction is completed, diethylamine and triethylamine being formed at the same time. To separate these bases from each other, the author recommends their conversion into picrates, the triethylamine salt being extremely insoluble, the salt of diethylamine extremely soluble, and the solubility of ethylamine picrate intermediate. To separate the bases from ammonia, the mixture is converted into sulphates, the solution is evaporated at 250° F., the pasty mass is exhausted with strong alcohol, and the resulting solution is again evaporated. To separate the bases, the mixed amine sulphates were distilled with caustic potash, the bases obtained were saturated with picric acid, and the solution allowed to crystallize. The first crystals were triethylamine picrate, the second ethylamine picrate, and the third diethylamine picrate, in the order of solubility.

In a paper on the "Reactions of Ethylamine and Diethylamine," the author states at considerable length the effects produced upon these bases by various metallic salts, and also gives the results of some experiments on their isomorphism.

He afterwards took up the study of the production of the methyl bases by heating together strong ammonia and methyl nitrate, as in the case of the ethyl bases. The chief product was methylamine. For the preparation of the methyl nitrate he found the use of urea, dissolved in the methyl alcohol, a most satisfactory modification. Subsequently he describes the separation of methylamine and its reactions, mentioning the salts methylamino-chloride of palladium and chloro-palladite of methylamine. In a later paper he describes triethylamine, its production, properties and reactions, and some of its salts.

In obtaining the urea for the preparation of the ethyl and methyl bases, Mr. Lea examined carefully the details of preparation of this substance from ferrocyanide of potassium, and by effecting a more complete oxidation by the use of a larger quantity of red lead he obtained as much as 500 grams of urea from 850 grams ferrocyanide.

In 1861, while preparing naphthylamine by the reduction of nitronaphthalin, he observed that if heat be applied before adding the caustic alkali a distillate is obtained which has a pale reddish color and which possesses the disgusting odor of naphthylamine. Mineral acids change its color to pale violet. Heated with sulphuric acid it becomes a rich blue-purple, and deposits after a time a small quantity of a black crystalline precipitate. Collected on a filter, this precipitate appeared in the form of nearly black needles with a most brilliant golden green glitter. On solution in alcohol, and evaporation, it was obtained as a dark red powder, which, when placed on glass and a platinum spatula drawn over it, gave a bright green, almost metallic reflecting surface, contrasting strongly with the red powder around it. The alcohol solution is of an intense dark blood red color, changing on the addition of a small quantity of sulphuric or nitric acid to an intense blue purple. For this new coloring matter he proposed the name *ionaphthine.*

Subsequently Carey Lea described another colored derivative of naphthalin, obtained in the course of preparation of the chloride by passing chlorine over it. On washing the crude product with ether and filtering, the ether, on evaporation, left a small quantity of a pale-yellow acid liquid, which after a time deposited a bright blue film, insoluble in water, alcohol, and ether. Exposed to ammonia gas, it became deep purple, the blue color being restored by acids.

In a paper published in 1865 he calls attention to a new reaction of gelatine, the first ever described as produced between pure gelatine and a perfectly colorless reagent. When a piece of gelatine is dropped into a solution of pernitrate of mercury it gradually assumes a strong red coloration and after a time dissolves completely to a fine red solution. On boiling for some minutes, its color deepens; but it is quickly decolorized by chlorate of potash. Metagelatine, prepared by allowing gelatine to swell in a cold saturated solution of oxalic acid, then heating moderately until the mass remained fluid on cooling, and removing the oxalic acid by carbonate of lime, was found to give the red coloration even more decidedly than ordinary gelatine.

In 1872 Mr. Lea published a criticism on a proposed method of estimating ethylic alcohol when present in methylic alcohol. The method is based on a determination of the melting point of methylic oxalate prepared from the methylic alcohol to be tested. If ethylic alcohol be present the melting point of the oxalate, which when pure is about 104° F., is claimed to be lowered. The author noticed that when well dried on blotting paper the crystals of methylic oxalate had precisely the same melting point, whether prepared from nearly pure methylic alcohol or from a specimen containing up to ten per cent. of ethylic alcohol. Evidently the crystals, when only squeezed, contain ethylic oxalate in the mother liquor, which of itself tends to lower the fusing point. The solidifying point observed experimentally would indicate the presence of only 3 or 4 per cent. of ethylic alcohol, whereas there was actually present in the sample 10 per cent. Evidently the method is not reliable.

In a paper on the detection of hydrocyanic acid Mr. Lea gives a new test for this substance. If a pure protosalt of iron, such as ferrous-ammonia sulphate, mixed with a little uranic nitrate, be dissolved in water, the solution gives, with a soluble cyanide, a purple precipitate, which in very dilute solutions is grayish purple. The solution should be quite neutral and nearly colorless. It is best made by mixing two solutions, each containing a grain or two of the respective salts in half an ounce of water. Two or three drops of the mixed solution are placed in a white porcelain capsule, and a drop or two of the liquid to be tested is allowed to trickle slowly down into it, noting the reaction at the point of contact. In this way the reaction obtained with a solution of potassic cyanide containing only one five-thousandth of a grain of hydrocyanic acid may be made to give a perfectly distinct reaction. As to the Prussian-blue reaction, the author points out that it is much more delicate than has been claimed, provided it be properly applied. If a weak ferrous solution contaimining a little ferric ammonia citrate be acidified with hydrochloric acid, and a few drops be placed in a white porcelain capsule, and a drop of the liquid to be tested be allowed to pass down the side of the capsule into it, a blue cloudiness will appear at the point of contact. In the author's experiments a distinct blue coloration appeared with a solution containing one grain of cyanide in four ounces of water, the drop containing, therefore, the equivalent $\frac{1}{5000}$ of a grain of hydrocyanic acid.

Besides his purely chemical papers, Carey Lea published many others in the domain of physics. As early as 1860 he called attention to the optical properties of picrate of manganese. This salt possesses markedly the remarkable property, pointed out by Brewster and Haidinger, of giving a colored ray by reflection, polarized perpendicularly to the incident plane, the color being quite different from that of the crystal itself. The picrate crystallizes in large and beautiful transparent right rhombic prisms, sometimes amber-yellow, sometimes aurora-red. Its optical properties are interesting, as it exhibits a beautiful dichroism. When viewed by light transmitted in the direction of its principal axis, it appears of a pale straw color; but in any other direction it is either a rich aurora-red in some specimens or a salmon color in others. Moreover, it possesses in a high degree the property of reflecting two oppositely polarized beams, which, since large crystals can easily be obtained, renders its examination easy. If a crystal be viewed by reflected light while it is held with its principal axis in the plane of incidence, the reflected light is not pure white, but shows a purple color. Examined with a dichroscope having its principal axis in the plane of incidence, the ordinary image is white, as usual, while the extraordinary one is purple. The crystals of picrate of manganese vary in color considerably. Those obtained by boiling picric acid with aqueous solution of cyanhydroferric acid and saturating with carbonate of manganese are of a deep rich color and show the purple polarized beam particularly well. This peculiar optical property should have a distinct name; and as the terms dichroism and pleochroism are used for transmitted light, the author proposes the term *catachroism* to express the property of reflecting two beams, one normally polarized in the plane of incidence and the other polarized in a plane perpendicular to it.

In 1861 Sprengel had devised an air blast for laboratory use founded on the well-known Catalan trompe (*Am. J. Sci.*, II, xxxii, 425). The following year Mr. Lea conceived that the principle might be made use of for aspirating as well as for blowing, and he described an apparatus performing both functions simultaneously, and admirably adapted to the purpose. This apparatus, in the form of the Bunsen filter pump, has since come into general use, especially for laboratory purposes; and, as modified and improved by Crookes, using mercury in place of water, it has made possible to science the high vacua which he has so thoroughly studied, and to electric lighting the construction of the incandescent lamp.

An ingenious paper by Mr. Lea was published in 1869 on certain phenomena of transmitted and diffused light. Having observed that when a beam of sunlight is thrown upon a white screen at fifteen or twenty feet distance, and a plate of finely ground glass is interposed, the white light acquires a deep orange-yellow color, he set himself to investigate the phenomenon. Three aspects of it were observed: First, where a strong beam of yellow, red, or reddish-yellow *direct* light is produced without the complementary blue being visible; second, where the yellow or red direct beam is visible and simultaneously the blue, the latter diffused; and, third, where reddish and bluish light, both diffused, are simultaneously visible. He concludes that all these results are due to interference and are capable of satisfactory explanation upon this hypothesis. To obtain the first result, take an ordinary plane silvered mirror, six by eight inches, and place it horizontally in a large beam of sunshine in a darkened room, so that the light is reflected to the wall or ceiling fifteen or twenty feet distant. Interpose, now, in the path of the ray after reflection, a plate of very finely ground glass. The ray which now passes through the colorless glass is no longer white, but deep yellow. As it is difficult to get glass finely enough ground, he suggests gelatinous alumina in a film of collodion or even copper hydrate in such a film, this latter arrangement showing the apparent paradox that colorless white light is dyed of a deep orange by passing through a bright blue film. The second result is obtained by the use of amorphous sulphur.

A few drops of sulphuric acid is added to fifteen or twenty ounces of water, and then two or three drops of ammonium sulphydrate. The sulphur, which separates in a few minutes, remains suspended in the liquid. Placed in a glass trough with parallel sides, a beam of sunlight sent through it and received on a screen fifteen or twenty feet distant is of a deep yellow color, while the liquid itself presents a rich blue tint. To obtain the third appearance, sago is made into a translucent paste with hot water largely diluted and placed in a flat gutta-percha dish, forming a layer at least an inch in depth. When an oblique beam of sunlight coming through a half-inch hole in the shutter falls upon the surface, it forms a bright oval surrounded by a halo two or three inches in diameter. The half of the halo farthest from the window is yellowish red, the other half bluish. To explain these phenomena the author considers one of the very small abrasions of the surface of ground glass. At its edge two parallel rays pass, one through the abrasion, the other through the original glass, the latter having traveled through a longer path in glass. If the index of refraction of the glass be 1.5, this ray will be retarded in the proportion of 1.5 to 1 for a distance equal to the depth of the abrasion. When this distance is extremely small, the blue rays will interfere. When large and of varying sizes, the various colors will produce white, as in the case of ordinary ground glass. If the abrasions are extremely small and very close, an excess of red light will be produced. Similar considerations apply to the second and third cases given above.

In 1896, shortly after the remarkable discovery of the Röntgen rays, Carey Lea made a series of very careful experiments to ascertain if these rays were detectable in sunlight. A very sensitive dry plate was placed in a book so that one hundred leaves and the red paper cover were between the sensitive film and the sunlight. A thick lead star was fastened to the outside of the cover and the whole exposed to bright sunshine for about seven hours. Not a trace of an image of the star appeared on development. Since Röntgen found his rays to penetrate one thousand printed pages, and moreover to traverse several meters of cardboard, the author's result is significant. A piece of sheet aluminum 1.2 millimeters thick was fitted into a frame, a very sensitive plate was placed behind it, and a lead star in front.

Not a trace of an image was obtained with three hours' exposure. Using a piece of white pine three-sixteenths of an inch thick, images were obtained by three minutes' exposure. On direct examination a panel of white pine one-fourth of an inch thick was observed to transmit red light. With full sunlight shining on thick card-board, a barium platinocyanide screen showed no indications of fluorescence. A thick disk of card-board, covered on one side with platinocyanide of barium, was placed in a tube which was directed to the sun. It showed fluorescence, but the interposition of very thin platinum foil cut off all the effect. Since Poincaré has expressed the opinion that all phosphorescent bodies emit Röntgen rays as well as light rays, the author exposed a dilute solution of uranin to sunlight, placing over it a sensitive plate protected by aluminum foil 0.1 millimeter thick, with a lead star interposed. An exposure of two hours gave no result. Repeated with quinine, five hours gave nothing. Rays from the Welsbach mantle, both with and without a chimney, were examined, but with no result. No rays capable of passing through aluminum foil 0.1 millimeter thick were detected by an exposure of five hours.

By far the most valuable as well as the most extended investigations of Carey Lea, however, were those which related to the chemistry of photography, in which at the time of his death he was the acknowledged authority. His researches were directed chiefly to the chemical and physical properties of the silver haloid salts, not only alone, but also in combination with each other and with various coloring matters, especially with reference to the action of light upon them under all these different conditions.

In one of Carey Lea's earlier papers, published in 1865, he gives a series of experiments which seem to him to decisively close the controversy then in progress in favor of the physical theory—*i. e.*, the theory that the change which takes place in an iodobromized plate in the camera is a purely physical one; that no chemical decomposition takes place, and hence that neither liberation of iodine nor reduction of silver results. In the first experiment he exposed and developed an iodobromized plate as usual, but instead of removing the unchanged iodide and bromide by fixing, he removed by means of a weak solution of acid mercuric nitrate the developed image without affecting the iodide and bromide. Obviously if the iodide or bromide had been in any way decomposed to aid in forming the developed negative image, there should have been left, on removing this, a more or less distinct positive image. Nothing of the sort was visible. The film was perfectly uniform, looking exactly as it did when it left the camera. In the second experiment the plate was treated similarly, except that the application of the mercuric nitrate was made in yellow light. It now showed nothing but a uniform yellow film; but, after washing, the application of an iron developer containing silver nitrate and citric acid produced the original image with all its details. As every trace of a picture as well as all reduced silver had been removed by the mercuric nitrate, it would seem to be absolutely demonstrated that the image is a purely physical one, and that after having served to produce one picture, that picture may be dissolved off and the same physical impression made to produce a second picture by a simple application of a developing agent.

In 1866 he prepared, by request, a résumé of a series of investigations whose object was to fix with greater exactness the obscure chemical and physical phenomena which lie at the basis of the photographic art, the details of which had been already published in the photographic journals. While it may be conceded that silver chloride and bromide undergo reduction while exposed to light, opinions differ as to the iodide, some holding that the action of light upon this haloid is purely physical; others that the change is an absolutely chemical one, reducing it either to a sub-iodide or even to metallic silver. In presence of free silver nitrate, reduction does in fact take place; but the important question now is, Does reduction of any sort invariably accompany the action of light upon silver iodide? Having produced specular films of pure metallic silver on plates of glass, iodizing them thoroughly, washing, exposing, and developing, and thus obtaining visible impressions, he conclusively proved that pure silver iodide is always sensitive to light. This established, he proceeded to the main question, Does chemical decomposition necessarily accompany the production of an impression upon silver iodide? In his view it does not. Pure silver iodide, he maintains, when exposed to light, receives a physical impression only; but if certain other substances, such as silver nitrate, tannin, etc., are present, then a chemical action, a reduction, may take place. In proof of the first statement a glass plate supporting a film of pure silver iodide was exposed to sunlight for many hours, then enclosed in a dark closet for thirty-six hours, then placed under a negative and exposed to light for two seconds. On pouring a developer over it a clear, bright picture instantly appeared. Hence the action of the sun for many hours had produced an impression which disappeared completely in thirty-six hours. Now, if the action of light is to reduce the iodide to sub-iodide, how did this sub-iodide recover its lost proportion of iodine? The fact that the iodide was much more powerfully affected by a recent exposure of two seconds than by one which, though thirty-six hours old, was many thousand times as strong, and in light much more intense, seems fatal to the chemical theory. By means of other direct and indirect experimental evidence he concludes that the action of light upon pure isolated silver iodide cannot be a chemical reduction: First, because the effect, even when carried many hundred thousand times further than in ordinary photographic processes, perfectly disappears in a few hours spontaneously, under circumstances which render it impossible to suppose that iodine could have been restored to replace that which must have been disengaged had any reduction taken place; second, because, even where the action of light is prolonged many hundred-thousand-fold the ordinary time, no reduced silver nor sub-iodide can be detected as present; third, because another metal, such as mercury, is capable of developing these images as well as silver; fourth, because a purely physical cause, to wit, mechanical pressure, is capable of producing a developable impression; and, finally, because there is not a single well-verified experiment which can be brought forward in support of the chemical view.

In a paper entitled "Contributions Toward a Theory of Photochemistry," published in 1867, Mr. Lea develops a remarkably ingenious theory based on the phenomena observed in silver iodide, which are the key to the whole subject. When this iodide is exposed to light it acquires a new property, that of attracting to itself a metallic precipitate in the act of forming, or a metallic vapor ready formed. A film of this iodide exposed for many hours to a bright sun does not further darken beyond the change produced by the first instants of diffuse light; and then if put aside in the dark for a brief time it re-acquires the capacity to receive an image by exposure for a second. What, then, is the nature of this change—this impression received in a second and then slowly passing spontaneously away? The answer Mr. Lea finds in the phenomena of phosphorescence. When silver chloride is exposed to light it becomes violet in color, losing one-half its chlorine—*i. e.*, it is decomposed; but when silver iodide is thus exposed no chemical change takes place, but the impression is for a time persistent. "The 'physical' impression of light is a persistence of the invisible (or 'chemical') rays exactly parallel to the persistence of visible or luminous rays in phosphorescence." In both cases the vibrations excited by light are not given out instantaneously, as in most light phenomena, where the return to photo-equilibrium is immediate, this equilibrium being reached only after an interval of hours or days. Evidently so long as these vibrations of non-luminous light continue, in the case of the iodide, for example, the body under their influence is much more disposed to suffer decomposition than when under normal influences; and if a film of such material has parts exposed to light, while other parts have been protected, and the whole be exposed to influences provocative of decomposition, it is evident that those influences may be so graduated that they will tell only upon the parts predisposed by the impression they have received. For this function of light, the existence of which produces the physical change suffered by exposed silver iodide, the author proposes the term *actinescence*. Just as in the case of phosphorescence, a body temporarily retains light and subsequently emits it, this emission being rendered evident by luminous phenomena, so by actinescence we have the phenomena described by Niepce de St. Victor as the storing up of light, where certain objects exposed to light and then carried into darkness have acquired the power of acting chemically upon other bodies with which they were placed in contact. Silver iodide when exposed to light in a state of perfect purity and isolated from all other substances undergoes no chemical change; carried into darkness it continues to vibrate in unison with the more highly refrangible rays, either those entirely beyond the visible spectrum or else those having a very low illuminating power, and this in so faint a degree that no phosphorescence is visible. If now it be brought into contact with any substance which would have occasioned decomposition in presence of light, then so long as this phosphorescence of actinic rays—this actinescence—continues' the same decomposition will take place. If simply left in the dark the actinescence will gradually expend itself precisely like ordinary phosphorescence. So, in both cases, a fresh exposure to light will create a fresh impression, the impressed material having perfectly recovered its original condition. "This theory," he says, "rests upon two properties for whose existence I have long contended, and which I believe I have succeeded in establishing, namely, the sensitiveness to light of iodide of silver even when perfectly isolated, and its spontaneous recovery of that sensitiveness after obliteration through powerful action of light, by simply remaining in darkness." He therefore concludes "that the latent image is simply due to a phosphorescence of chemical or actinic rays, to which property I give the name of *actinescence.*

In a paper read before the American Association for the Advancement of Science at Newport in 1868, Mr. Lea pointed out the well-known fact that silver bromide, when exposed to light, undergoes decomposition, with elimination either of bromine or a bromine compound, being at the same time reduced to subbromide, the result being a distinct darkening. In his experiments organic matter was eliminated by forming the film on a plate of glass, first by silvering it and then treating it with bromine or iodine. On exposure for four hours the pure and isolated bromide film gave a distinct impression. Since silver iodide is not thus decomposed, the latent image produced by its exposure to light must be purely physical. The object of the paper was to show that silver bromide is also capable of forming a latent image, in which chemical decomposition plays no part, and which therefore must be considered as a mere molecular alteration, a physical as distinguished from a chemical image. This physical image, however, is quite different from that formed on silver iodide. While on the latter the physical image is produced only when the iodide is isolated from all other bodies, that on the bromide is found only in the presence of organic matter; and, secondly, while the physical image on silver iodide can be called forth only in the presence of silver or of some other metallic body, this image on silver bromide can be developed in the complete absence of any metallic body. Let a collodion film containing silver bromide and free silver nitrate be formed on glass, washed, plunged into a solution of tannin, and dried. Expose this for a short time in the camera. A latent image is formed. Place the plate in pyrogallol solution and the image appears. But how is this? It cannot be that an infinitesimal chemical image of sub-bromide, acting as a nucleus, was brought up to a visible intensity by the action of the developer, because pyrogallol alone has no power to do this, and because free silver nitrate must also be present, and this had been removed in the washing. The only alternative is that that portion of the film upon which the light had acted was so modified thereby that it was brought into a condition to be more easily decomposed by pyrogallol than the portion which had not been acted on. Now, if portions of the bromide film not decomposed by light, but simply acted on by it, are subsequently decomposed by the action of pyrogallol, while those portions of the film not influenced by light are not decomposed by the pyrogallol, then it follows that the action of the light is so far simply physical.

In 1878 Carey Lea sought to determine the precise amount of material actually altered by the action of light. Silver chloride, precipitated with excess of hydrochloric acid, and well washed, was exposed to bright sunlight for five days. Two grams of the dark powder were thoroughly treated with sodium hyposulphite to remove the unaltered chloride. The residue weighed twenty-one milligrams, showing that only about 1 per cent. of the chloride had been acted on. If we suppose the action to consist in the removal of one-half the chlorine, the whole loss in weight by the action of the light would be only a little more than one-tenth of 1 per cent. As the best reagents for removing the unaltered chloride are liquid ammonia and sodium hyposulphite, and as both of them attack the altered substance, the difficulty in verifying the nature of the action of light is very considerable. Nitric acid does not attack the darkened portion of the silver chloride in the least, while the dark residue left by the above reagents is instantly dissolved by cold nitric acid with evolution of red fumes. Evidently no metallic silver is produced by the action of the light, while it is produced by the subsequent action of the ammonia and the hyposulphite. Since the black substance is made white by *aqua regia*, it evidently contains less chlorine than the chloride, and so may be either a subchloride or an oxychloride.* The substance produced by the action of light on silver chloride is of a much more permanent character than in the case of the other silver haloids.

When silver iodide is blackened under ammonia solution in a test-tube and set aside in the dark for a day or two, the iodide assumes a singular pinkish shade. Hence it appears that silver iodide, under the influence of ammonia and of light, gives indications of most of the colors of the spectrum. Starting with white, it passes under the influence of light to violet and thence nearly to black. This violet-black substance, washed with water, passes to brown. The brown body, covered with ammonia and left to itself in an open test-tube, becomes pinkish in the dark, yellow in sunlight. These curious relations to color which we see in the silver haloids seem to give hope of the eventual discovery of some complete method of heliochromy.

In 1874 Carey Lea made an extended series of experiments to test a theory advanced shortly before by Dr. Hermann Vogel, to the effect that substances placed in contact with silver bromide modify its impressibility by rays of different refrangibilities. Moreover, he maintained that the change will follow a certain law—*i. e.*, that colored substances absorbing certain rays will increase the impressibility of the bromide to those rays which they absorb. Thus a colored substance which absorbs the yellow rays and radiates the rest of the spectrum will increase the sensitiveness of silver bromide to the yellow ray. Mr. Lea's results seemed to establish the fact that there is no general law connecting the color of a substance with the greater or less sensitiveness which it brings to any silver haloid for any particular ray.

The following year Mr. Lea made an elaborate investigation on the action of the less refrangible rays of light on silver iodide and bromide. The effects upon the silver compounds were obtained by forming them in the body of pure paper, the silver solution being applied in all cases after that of the alkali-haloid, and the paper thoroughly washed. Colored glasses were used to obtain the colored light, in preference to the spectrum itself, the absorption spectrum of these glasses being exactly determined

* Proved later to be a subchloride.

with the spectroscope. Experiments were also made to test the theory of E. Becquerel, according to which light consists of two classes of rays, the "exciting" and the "continuing" rays. The more refrangible rays alone have the power of originating an impression; the less refrangible were powerless to commence, but were capable of continuing and reinforcing an impression which had been commenced by the more refrangible rays. The conclusions reached by the author as a consequence of the one hundred and sixty experiments made were:

1. Silver bromide and iodide are sensitive to all the visibie rays of the spectrum.

2. Silver iodide is more sensitive than the bromide to all the less refrangible rays, and also to white light.

3. The sensitiveness of silver bromide to the green rays is materially increased by the presence of free silver nitrate.

4. Silver bromide and silver iodide together are more sensitive to both the green and the red rays (and probably to all rays) than either the bromide or the iodide separately.

5. There do not exist any rays with a special exciting or a special continuing power, but all the colored rays are capable both of commencing and continuing the impression on silver iodide and bromide.

In a research published in 1876 Mr. Lea confined himself to the question: Does there exist any red substance which is capable of increasing the sensitiveness of silver bromide to the green rays? Of all the red pigments tested, corallin was the only one which produced the slightest increase of sensitiveness to the green rays. But for two good reasons this power cannot be considered as a function of its color: First, because it shows a still more marked tendency to increase the sensitiveness of silver bromide to the red ray than to the green; and, second, because it is easy to destroy its action on green light without destroying its color. Moreover, he observed that many entirely colorless substances gave an increase of sensitiveness to the green rays. So that it is not among the colored but among the colorless substances that we must look for those capable of enhancing sensitiveness to green light. Not a single red substance could be found that possessed that property, while no less than eight colorless substances exhibited it. The conclusion is the same as before: "There exists no relation between the color of a substance and that of the rays to which it increases the sensitiveness of silver bromide."

In 1877 Carey Lea pointed out that salts of silver may exhibit sensitiveness to light in three ways: They may exhibit a sensible darkening or they may receive a latent image; and this image may have a capacity of being rendered visible either by receiving a deposit of metallic silver or by decomposition by alkalies in connection with reducing agents. The last two phenomena are quite distinct, since in the first the image is produced entirely by the addition of silver not previously present, while in the second no silver whatever is added, that portion of substance which received the direct action of light undergoing decomposition by subsequent treatment. The action of light sets up a molecular change in both cases. In the one case the portions acted on by light become more apt to attract a precipitate in the act of formation; in the other they are more readily attacked by certain reducing agents. Of course the silver compounds which show the greatest tendency to form latent images by the action of light are the chloride, bromide, and iodide; but the same tendency exists, though in a less degree, in other compounds, as was shown many years ago by Robert Hunt, in the case of other silver salts. Out of twenty-four silver salts examined, the author found eleven which gave latent images capable of development by the action of pyrogallol and ammonia. Curiously enough, it was observed that no substance insensitive in the absence of tannin acquired sensitiveness by its presence.

Since the development of the latent or invisible photographic image produced by the action of light is, beyond all question, the most remarkable and the most interesting fact in photochemistry, it is surprising with what slowness our knowledge of the substances capable of producing this effect has increased. In 1877 Carey Lea made an extended examination of substances likely to act as developers. Since in general the effect is a reducing one, it is among reducing agents that these substances are likely to be found; but not only this—an elective power is needed; a tendency to reduce, not the whole surface, but only those parts that have been acted on by light, and to spare the others. The results show (1) that the number of bodies endowed with the power of developing the latent image, so far from being very limited, as hitherto supposed, is, on the contrary, very large; and (2) that potash acts more powerfully in aiding development than ammonia, contrary to the general opinion. Moreover, he observed that the use of free alkali is not necessary to the most energetic development, as has been supposed; and this led him to devise a form of development which, though there is no free alkali present, is more powerful than any yet known. He also disproved the common impression that ferrous salts act only in presence of a soluble salt of silver, and in its absence scarcely attack the silver haloid in the film to produce the image. Among the various developing substances examined, the salts of ferrous oxide proved to be the most interesting and remarkable of all in their action on the image; and of these, ferrous oxalate exhibits developing powers of a very marked kind. The same exposure which with alkaline pyrogallol gives a weak and sunk-in image after a protracted development gives with ferrous oxalate a bright, bold image and in much less time. The development is particularly clear and clean. The unexposed parts are not attacked. The developer possesses a great deal of that elective power previously mentioned, which causes it to react strongly on those parts which have received the influence of light, and to spare those which have not. Three years later the study of the developing power of ferrous salts was resumed and extended to include many inorganic compounds. The most active ferrous salts found were the borate, phosphate, sulphite, and oxalate.

The fact is well known that certain organic substances, tannin, for example, placed in contact with the washed haloid, increase its sensitiveness. Poitevin and Vogel proposed the theory that these substances acted in virtue of their affinity for the halogen. But Carey Lea pointed out soon afterward that the one property that these substances possess in common is that they are all reducing agents. Hence he concludes that their action is due to the fact not that they abstract the halogen, but that their affinity comes in to aid the affinity of the halogen for the hydrogen, and that under the influence of the light water is decomposed. According to this view, whenever silver iodide is exposed to light in presence of an organic body capable of accelerating the action of this agent, there should be formed traces of free acid; whereas the Poitevin-Vogel theory requires the formation of an iodo-substitution compound. To test the question, silver iodide precipitated with excess of potassium iodide and well washed, was exposed, after receiving a small quantity of pyrogallol, to sunlight for fifteen minutes in presence of water. The liquid, which was at first neutral, showed a distinct acid reaction at the end of fifteen minutes. Again, on the Poitevin-Vogel theory, substances having an affinity for iodine should increase the sensitiveness, and substances not having it should have no such action. But this is not the fact. Substances like potassium carbonate solution and like starch, for example, which have an affinity for iodine, do not appear to increase the sensitiveness of silver iodide by contact with it. Hence he concludes that such organic bodies as increase the sensitiveness of the silver haloids to light do so not by forming substitution compounds with the halogen, but by promoting, in virtue of their affinity for oxygen, the decomposition of water by this halogen.

As early as 1866 Carey Lea had proved that the intense black substance produced by the action of light upon silver iodide in presence of silver nitrate contains iodine, and therefore is either a sub-iodide or an oxy-iodide. In 1874 he extended his examination to silver bromide. He observed that when silver bromide is treated with pyrogallol and alkali after exposure to light, the black substance contains bromine, and is resolved by nitric acid into normal silver bromide and silver, which latter is dissolved. It is therefore either a sub-bromide or an oxy-bromide, probably the former. In 1878 he proved the same fact for the black substance given by the chloride when acted on by light.

Perhaps the most important contribution made by Carey Lea to photochemistry was his discovery of what he termed photo-salts. His earliest paper on the subject appeared in 1885, and described the remarkable property possessed by the silver haloids of entering into chemical combination with certain coloring matters in somewhat the same way as alumina does, forming lakes. The freshly precipitated and still moist silver salt is brought into contact with the coloring matter, or it is precipitated in presence of it. The union takes place readily, and the color cannot be washed out. What is curious and seems to be evidence that the combination is intimate is the fact that the color assumed by the silver salt may differ considerably from that of the dye. The three haloids may even be differently colored by the same coloring substance. Generally, however, coloring matters impart their own shade or something like it to the haloid. The bromide precipitated in presence of silver nitrate takes from aniline purple a strong purple color, from cardinal red a bright flesh or salmon color, from naphthalin yellow a yellow color. Sometimes different specimens of the same coloring matter give different colors. Silver bromide received from one specimen of methyl green a bluish green, while another specimen of the same dye produced in it a deep purplish color. Already in 1868 he had proposed* to color or stain the photographic film in order to modify its behavior toward light—*i. e.*, to prevent blurring or irradiation. At that time the best color was found to be red litmus. The theory of Vogel that a film thus colored gained sensitiveness to those rays of the spectrum which the coloring matter absorbs is made improbable by the fact that the color in the film tends to arrest precisely those rays to which it is proposed to render the silver salt more sensitive. John W. Draper's view that substances sensitive to light are affected by the rays which they absorb seems *a priori* more probable. The author, however, observes that the effect will depend, first, upon the capacity of the dye to combine with the silver haloid, and, second, not on the proper color of the isolated dye, but on the color that the silver haloid acquires from it.

The paper containing his photosalt theory was published in 1887. Of the two theories of the latent photographic image, the physical and the chemical one, he had inclined at the outset to the first of these. Of late years results had been obtained not readily reconcilable with it. On the other hand, the theory that the latent image is formed of subsalts is open to the objection that while subsalts are readily attacked by nitric acid, the latent image may be exposed without effect to this acid. Three years of laboratory work had led him to a truer theory, based on the fact that the silver haloids are capable of uniting with coloring matters to form stable compounds. He now finds that in much the same way a silver haloid may unite with a certain proportion of its own subsalt to form colored compounds, which by this union quite loses its characteristic instability and yields a compound of great permanence. When now silver chloride,

* British Journal of Photography, xv, 210, 506, 1868.

bromide, or iodide contains as little as one-half of one per cent. of subsalt combined with it, its properties are greatly changed. It has a strong coloration and its behavior to light is altered. It is one of the forms of this substance which constitutes the actual material of the latent photographic image. Of the three, the chlorine salt is the most stable and exhibits the finer variety of coloration. Hence it is the most interesting because of its relations to heliochromy. It shows all the warm shades from white to black through the following gradations: white, pale flesh color, pale pink, rose color, copper color, red purple, dark chocolate black. These compounds are obtained in an endless variety of ways—by chlorizing metallic silver; by acting on normal chloride with reducing agents; by partly reducing silver oxide or silver carbonate by heat and treating with hydrochloric acid, followed by nitric acid; by acting on subchloride with nitric acid or an alkaline hypochlorite; by attacking almost any soluble salt of silver with ferrous, manganous, or chromous oxide, followed by hydrochloric acid; by reducing silver citrate by hydrogen and treating it with acid; by treating a soluble silver salt or almost any silver solution with potash or soda and almost any reducing agent—cane sugar, milk sugar, glucose, dextrine, aldehyde, alcohol, etc.—and supersaturating it with hydrochloric acid. So, also, almost any salt of silver exposed to light, treated with hydrochloric acid and then with strong nitric acid yields it. Since these substances have been seen hitherto only in the impure form in which they are produced by the continued action of light on the normal salts, the author proposes to call them *photosalts*; as *photochloride*, *photobromide*, and *photoiodide*.

In the second part of his paper the author gives evidence to prove that the strongly colored photosalts now described, obtained independently of any action of light, are identical, first, with the product obtained by the continued action of light on these haloids, and, second, with the substance of the latent image itself. If silver chloride precipitated with excess of hydrochloric acid be exposed to light, we get a deep, purple-black substance which, when boiled with dilute nitric acid, gives up a little silver and becomes a little lighter, changing to a dull purple, resembling closely some of the forms of photochloride already described, chiefly those produced by the action of sodium hypochlorite or of ferric chloride on metallic silver. When silver oxalate is covered with water and exposed to sunshine for two days, being frequently agitated, it changes to a deep brownish black, becoming a little lighter by treatment with hydrochloric acid. When washed and boiled with nitric acid it acquires a fine deep copper-red color. A sample especially prepared in this way which had a fine lilac purple color was found to contain one-half of 1 per cent. of subchloride. The red chloride thus obtained by the action of light on silver oxalate not only resembles closely the red chloride obtained by means exclusively chemical, but shows the same behavior to reagents. Other silver salts were examined, and it was found that all of them thus treated yielded pink or red photochloride. In considering the question of the latent image the author calls attention to the remarkable fact that a dilute solution of sodium hypophosphite if poured over a mass of silver chloride, bromide, or iodide formed in the absence of light, produces no visible effect, but has the property of bringing those substances into the condition in which they exist in the latent image. Applied in strong solution with the aid of heat it produces brown photochloride, photobromide, or photoiodide of silver. Experimental evidence is given to show, first, that in the entire absence of light sodium hypophosphite is able to affect a sensitive film of silver haloid exactly in the same way as does light, producing a result equivalent to a latent image formed by light and capable of development in the same way as an actual impression of light; second, that these two effects, the impression produced by hypophosphite and that by light, comport themselves to reagents exactly in the same way, and seem in every way identical; and, third, that the image produced by hypophosphite on silver chloride always gives rise to a positive development, but on silver bromide may give rise either to a direct or to a reverse image, both of these effects corresponding exactly with those of light. But more than this. Sodium hypophosphite may be made to reverse the image produced by light on silver bromide and, conversely, light may be made to reverse the action of hypophosphite. It would seem, therefore, that the question of the identity of the photosalts with the products of light on the silver haloids may, perhaps, with some confidence be allowed to rest on the cumulative proofs here offered.

In connection with the papers just referred to, Mr. Lea published soon afterwards a suggestive communication on what he calls "Image Transference." He had already shown that it was possible to make marks with sodium hypophosphite upon a silver haloid film, and then to obtain a development of these marks precisely as if they had been impressed by light, but of course quite independent of any exposure thereto. He now proves that it is possible to develop on a film of silver haloid a complete image—a print from a negative, for example—without exposing the silver haloid either to light or to the action of hypophosphite or subjecting it to any treatment whatever between the moment of its formation and that of its development. The film of silver comes into existence with the image already impressed upon it. A film of silver salt, citrate, benzoate, tartrate, pyrophosphate, etc., is formed on paper in the ordinary way, and this is exposed, under a negative, to sunshine for a few seconds. Then by plunging the film into dilute hydrochloric or hydrobromic acid it is converted into silver chloride or bromide. The acid is then washed out; and now, on putting the film, thus converted into silver haloid, into a ferro-oxalate developer, the image appears at once. The silver chloride or bromide comes into existence with the image already impressed upon it at the instant of its formation; so that, although the substance which received the image is completely broken up and destroyed, the image is not, but is transferred in all its details to the new film of silver haloid. It is therefore evident that the action of light on all silver salts that can thus transfer an image must be similar in all its essentials to the action of light on the silver haloids. Hence it follows that all such silver salts must be capable of forming subsalts, else the image could not be transferred.

In further support of his position that the principal and characteristic product of the action of light on the silver haloids is a combination of the haloid with a small proportion of its own subsalt, the author in 1887 offered direct evidence of the fact that silver chloride can unite with small quantities of other metallic chlorides. "That an actual combination, though one quite outside of atomic proportion, takes place, is proved by two facts: first, that the chloride with which the silver haloid unites, though soluble in water, is not removable by water; and, again, that the properties of the haloid are markedly changed." Thus if ferric chloride is added to dilute hydrochloric acid, and then silver nitrate, the silver chloride precipitated is not white, but buff colored. The ferric chloride cannot be removed by washing, and even hydrochloric acid removes it only in part; but even this minute quantity of iron profoundly affects the sensitiveness of the silver salt to light. In a comparative test the normal silver chloride had passed to a full violet with an exposure which produced on the other scarcely any visible effect. Similar results were obtained with cobalt, nickel, and manganese.

As long ago as 1878 Mr. Lea had pointed out that as the black substance produced by the action of light upon silver chloride became white on treating it with *aqua regia*, it evidently contained less chlorine than the chloride, and so must be either a subchloride or an oxychloride. Subsequent investigations immensely strengthened the presumption that it was a subchloride, especially the remarkable discovery of photosalts and their rôle in photography. Since, however, advocates of the oxychloride theory still-existed in 1889, the author undertook to settle the question by what he calls proof by exclusion. Silver chloride was precipitated with excess of hydrochloric acid, washed in a darkened room, and dried in a desiccator. It was then placed in a porcelain crucible, fused, and poured into naphtha in a vessel carefully freed from moisture. The chloride solidified to a pale-gray lump, which, while in the naphtha, was absolutely free from all possibility of contact with oxygen. On moving the vessel into the sunshine the chloride instantly became as black as ink. In a second experiment the converse method was used. Pure silver, reduced by cadmium from the chloride, was heated nearly to redness in a porcelain capsule and dropped into naphtha. Some fragments of iodine were added. The action, though slow, was continuous and regular. After some hours the whole of the iodine had united with the silver to form a black compound. It is clear, therefore, that, whether we start from silver chloride and proceed by reduction, or from metallic silver and proceed by iodization, we can, in either case, obtain a photosalt under conditions which rigorously exclude all possibility of the presence of oxygen or of moisture in any shape. The photosalt, therefore, is not an oxysalt, but a compound of normal salt with subsalt.

In further proof of the existence of hemi-compounds—*i. e.*, of compounds intermediate between the normal salts and the metal, such as the hemihaloids of silver, which play so important a part in the photosalts—Mr. Lea published, in 1892, a paper on silver hemisulphate, which he had obtained as a double salt of hemisulphate and normal sulphate containing one molecule of each. The new salt has a bright brown color and has a stability which, in view of its composition, is remarkable. Nitric acid, unless very strong, has but little action on it. Ferrous sulphate, which instantly reduces silver sulphate, has no action on it whatever. Hot sulphuric acid is without action. The new substance was formed by the joint action of sulphuric and hypophosphorous acid on a silver salt, the silver sulphate being formed in presence of the hypophosphorous acid. Either the nitrate, phosphate, or carbonate may be used.

Perhaps the most remarkable discovery made by Carey Lea is that of allotropic silver. Early in the year 1886 he had taken up the study of the reduction products of silver in connection with that of the photosalts. At first the results were most enigmatical, but eventually stable products, capable of a fair amount of purification, were obtained. The reaction employed was the reduction of silver citrate by ferrous citrate. Even the earlier and less pure forms of allotropic silver thus prepared were exceedingly beautiful; the purer are hardly surpassed in this respect by any known chemical products. The forms obtained he classifies as: A, soluble, deep red in solution, mat-lilac, blue, or green while moist, brilliant bluish green, metallic when dry; B, insoluble, derived from A, dark reddish brown while moist, when dry somewhat resembling A; C, gold silver, dark bronze while wet, when dry exactly resembling metallic gold in burnished lumps. Of this form there is a variety which is copper-colored. The C form is insoluble in water and appears to have no corresponding soluble form. All these forms have several remarkable properties in common. These are: (1) that of drying with their particles in optical contact, and consequently forming a continuous film; C, when so treated, would be taken for gold-leaf; on glass, an absolutely perfect mirror is obtained; (2) that of taking a very beautiful coloration when brushed over paper and exposed to the action of any haloid solution; with sodium hypochlorite the results are often magnificent, giving intense shades, with metallic reflections, reminding one of the colors of a peacock's tail; (3) that of being converted by the stronger acids, even when much diluted, into normal gray silver—a change which takes place absolutely without any separation of gas; and (4) that of being easily reduced to an impalpable powder. To see what is apparently solid burnished metal break easily in pieces and yield a fine powder by moderate trituration is surprising. In preparing the A form of allotropic silver, concentrated solutions of ferrous citrate and a silver salt are mixed together. The liquid turns almost completely black and deposits in 10 or 15 minutes a heavy precipitate of a fine lilac-blue color. When thrown on a filter it takes a deep blue color, without losing its solubility. In water it dissolves with an intense blood-red color, but it is insoluble in a 5 or 10 per cent. solution of sodium nitrate, citrate, or sulphate, or of ammonium nitrate. After several re-precipitations and washings, first with ammonium nitrate and then with alcohol, and drying, three analyses gave 97.31, 97.18, and 97.21 per cent. of silver, the other 2 or 3 per cent. being shown to be ferric oxide and citric acid as an accidental impurity. The inference seems to be very strong, therefore, that there exists an allotropic form of silver freely soluble in water. From this solution it is readily precipitated by the addition of almost any neutral substance, in either a soluble or an insoluble form. Alkali sulphates, nitrates, and citrates throw down the soluble form; magnesium, copper, ferrous and nickel sulphates, and even silver nitrate, throw down a perfectly insoluble form of a purple-brown color. This insoluble form may be made to return to the soluble condition by a dilute solution of sodium borate, which gives a brown solution; by one of sodium sulphate, which gives a yellowish red one, and by one of ammonium sulphate, also a red one. The insoluble variety is soluble in ammonia, giving a fine red solution. The gold yellow and copper-colored silver is obtained by mixing solutions of silver nitrate, ferrous sulphate, and Rochelle salt in definite proportions. A powder, at first glittering red, then changing to black, is thrown down, which on the filter becomes of a beautiful bronze color. It is spread on a watch glass and allowed to dry. It forms lumps exactly resembling highly polished gold. On analysis it gave 98.75 per cent. silver. Experiment showed the density of allotropic silver to be less than that of normal silver, the blue substance, B, giving 9.58, the yellow variety, C, 8.51, that of normal silver being about 10.5. Moist gold-colored silver is converted into normal silver by the action of light. As thus produced, it is exquisitely beautiful, having a pure and perfect white color, like the finest frosted jeweler's silver. All the forms of allotropic silver are sensitive to light. Their stability is very variable, and this under conditions so far difficult to define.

Mr. Lea subsequently points out that the three forms of allotropic silver above mentioned are not to be understood as the only forms which exist, but only as the most marked. The substance is protean. No other metal appears to be capable of assuming such a remarkable variety of appearances. Metallic silver has been obtained—blue, green (many shades of both), red, yellow, and purple. An intense yellow-brown solution of allotropic silver was changed by a little solution of disodium phosphate to bright scarlet, presently decolorizing with formation of a purple precipitate, which in its turn, when washed on a filter, changed to bluish green.

In his paper of March, 1891, Mr. Lea undertakes (1) to describe the reactions of gold-colored allotropic silver; (2) to show that a well characterized form of silver exists intermediate between the allotropic silver already described and ordinary silver, differing in a marked way from both; (3) to prove that all the forms of energy act upon allotropic silver, converting it either into ordinary silver or into the intermediate form; mechanical force and high-tension electricity converting it directly into ordinary silver, while heat and chemical action convert it, first, into the intermediate form, then into ordinary silver; light produces the intermediate form only; and (4) to show that there exists a remarkable parallelism between the action of these forms of force on allotropic silver and their action on the silver haloids, indicating that it is not improbable that in these haloids silver may exist in the allotropic condition. As to the intermediate form, he finds that if a chemically clean glass plate be coated with a film of gold-colored allotropic silver, allowed to dry, first in the air and then at 100° C. for an hour or two, and then, if the middle of the plate be carefully heated over a spirit lamp, there is obtained a whitish-gray circle with a bright, lustrous, golden-yellow ring about it, somewhat lighter and brighter than the portion of the plate that has not been changed by heat. This ring consists of what he proposes to call the "intermediate form." Its properties are better seen by using a film formed on pure paper, one end of which is heated over a spirit lamp to a temperature just below that at which paper scorches. The change is sudden and passes over the heated portion of the surface like a flash. On examining the changed part we find: First, that it has changed from a deep-gold to a bright-yellow gold color; second, that it does not whiten or change color in the slightest degree when subjected to a shearing stress; third, that it is much harder, as is readily perceived on burnishing it; and, fourth, that it no longer shows the color reaction with potassium ferricyanide and ferric chloride, changing only by a slight deepening of color. This paper concludes with an interesting résumé of the action of different forms of energy on allotropic silver, and discusses its color reactions with the aid of beautifully colored lithographic plates.

In his paper of April, 1891, Mr. Lea sums up his conclusions as follows: "That silver may exist in three forms: First, allotropic silver, which is protean in its nature; may be soluble or insoluble in water, may be yellow, red, blue, or green, or may have almost any color, but in all its insoluble varieties always exhibits plasticity—that is, if brushed in a pasty state upon a smooth surface its particles dry in optical contact and with a brilliant metallic luster; it is chemically active; second, the intermediate form, which may be yellow or green, always shows metallic luster. but is never plastic and is almost as indifferent chemically as white silver; third, ordinary silver." Further he points out "that allotropic silver can always be converted either into the intermediate form or directly into ordinary silver; that the intermediate form can always be converted into ordinary silver, but that these processes can never be reversed; so that to pass from ordinary silver to allotropic it must first be rendered atomic by combination, and then be brought back to the metallic form under conditions which check the atoms in uniting; that allotropic silver is affected by all forms of energy, and that this effect is always in one direction, namely, toward condensation; that the silver haloids are similarly affected by the same agencies; that a remarkable parallelism is noticeable between the two actions, especially if we take into account the fact that in the haloids the influence of energy is to some extent restrained by the strong affinity which the halogens show for atomic silver. There is therefore reasonable ground to suppose that in the silver haloids silver may exist in the allotropic form.

In the course of his investigations Mr. Lea had become greatly interested in the relations of energy to the chemical changes in matter. Since it is well known that when a substance is capable of existing in two allotropic forms and of being converted from one into the other by pressure, the body resulting from pressure is always the more dense of the two, is less active chemically, and is a polymer of the first, it should follow that allotropic silver, which is converted into normal silver by the simple pressure of the finger, should be less dense than it and should have a greater chemical activity. This Mr. Lea has shown to be the fact. In the case of the three forms of silver—the allotropic, the intermediate, and the ordinary form—he pointed out as early as 1891 that while the first form can be converted into the second and third in several ways and with the utmost facility, and that the second can also be converted into the third, these transformations can by no possibility be reversed. To convert ordinary into allotropic silver we must as a first step dissolve it in an acid—that is, convert it from a polymerized into an atomic form—since only from this atomic form can allotropic silver be obtained. Hence he suggests that the three forms of silver may be considered as atomic, molecular, and polymerized. Special experiments made upon the silver haloids showed that these compounds, though substances of very great stability, have their equilibrium so balanced as to respond to the slightest influence, not merely of light, but of any form of energy; not receiving a momentary but a permanent impression, which, though so slight as to be invisible, still greatly increases the tendency of the molecule to fall in pieces under the action of a reducing agent. It is not light only, therefore, that is capable of producing an invisible image. The power belongs alike to all forms of energy. In his paper read before the National Academy in April, 1892, he showed that not only heat, light, electricity, and chemism are capable of disrupting the molecule, but that mechanical force also is able to do this. Silver chloride was enclosed in platinum foil and exposed to a pressure of about one hundred thousand pounds to the square inch, maintained for twenty-four hours. The chloride was completely blackened except at its edges, where, of course, the pressure was less. Silver bromide gave the same result. Silver iodide is not blackened by light; but, to his great surprise, it darkened under pressure to the same extent as the others. Even shearing stress obtained by simple trituration in a porcelain mortar produced a darkening of silver chloride—a true silver photochloride. These observations prove the existence of a perfect uniformity in the action of all kinds of energy on the silver haloids. The balance of the molecule is at once affected by the influence of any form of energy. A slight application produces an effect which, though invisible to the eye, is instantly made evident by the application of a reducing agent. The bonds which unite the atoms have evidently been loosened in some way, so that these molecules break up more easily than those to which energy has not been applied. Hence, if the substance be submitted to the action of light, heat, or electricity, or if lines are drawn across it with a glass rod or with sulphuric acid, a reducing agent blackens the parts so treated before it affects the parts not so treated. Obviously the phenomena of the latent image and of its development are not especially connected with light, but belong to other forms of energy as well. It follows, therefore, that every form of energy is capable, not only of producing an invisible image—that is, of loosening the bonds which unite the atoms—but also, if applied more strongly, of totally disrupting the molecule. Mechanical force, even, is therefore competent without the aid of heat to break up a molecule which owes its existence to an exothermic reaction. Obviously this phenomenon has nothing in common with decompositions produced by mechanical force in silver or mercury fulminate and similar explosives. Such substances are all formed by endothermic reactions, and their decompositions are exothermic. But silver haloids are formed by exothermic reactions, and consequently their decompositions are endothermic and require the energy which was set free in their formation to be returned to effect their decomposition. The experiments now described show that mechanical force may be made to supply this energy, and so play the part of light, electricity, or heat without previous conversion into any of these forms.

In a paper on "Endothermic Reactions Effected by Mechanical Force," published in 1893, Mr. Lea generalized his proposition and sought to determine whether the same agent—*i. e.*, mechanical force—would not be capable of bringing about analogous chemical changes in other compounds. By means of a powerful press the following reactions, all endothermic, were produced: Silver sulphite, salicylate and carbonate, ammonium platino-chloride, and mercuric oxychloride underwent a well-marked darkening. Since these reactions are endothermic, it follows that mechanical force can bring about reactions which require an absorption of energy, which energy is supplied by the mechanical force precisely as light, heat, and electricity supply energy in the endothermic changes which they bring about.

In the second part of this paper Carey Lea not only adds to the number of endothermic reactions produced by mechanical force, but in one case has been able to obtain actual quantitative results, the mechanical force being applied in the form of shearing stress produced by sharply grinding the material in a mortar. Thus mercuric chloride, which did not darken in the least with ammonia, became gray very readily after fifteen minutes' trituration. Mercurous chloride became at first yellow and then blackened without difficulty. Mercuric iodide and oxide, platinic chloride, silver tartrate, citrate, and oxalate all blackened. Half a gram of sodium chloraurate gave in half an hour 10.5 mgrms. of gold. Calculation shows that the amount of energy absorbed in setting free the 10.5 mgrms. of gold would raise 518 grams to the height of one meter. Hence 518 gram-meters represent the amount of mechanical energy transformed into chemical energy in this experiment. The production of endothermic change by shearing stress can be very simply shown by soaking strong paper in a solution of the substance, very thoroughly drying it, then laying it upon a plate of glass and rubbing it strongly with the end of a glass rod.

In a third paper on this subject Mr. Lea gives additional examples of the transformation of mechanical into chemical energy. In one case half a gram of mercuric oxide was triturated in a porcelain mortar, and 30.5 mgrms. of mercury were obtained. Calculating the energy required to reduce this quantity of mercuric oxide to mercurous, it corresponds to 322 gram-meters, which is the amount of mechanical energy transformed into chemical energy.

In 1895 and 1896 Carey Lea published two ingenious papers on the Color Relations of Atoms, Ions, and Molecules. In the first of these, read before the National Academy in April, he points out (1) that the atoms of which elements are composed differ remarkably in color from the elements themselves, and (2) that their colors are more important and more characteristic than those of the elements. So that if we divide the entire series of elements into two classes, (*a*) those whose atoms are always colorless, whatever may be their valency, and (*b*) those whose atoms are either sometimes or always colored, we shall find that this division harmonizes in a striking way with their chemical properties. The fact is remarkable that it is never possible to deduce the color of an atom from that of the element which it forms by combining with another similar atom. Between the atom and the element there seems to be no color relation whatever. It is from the combinations of an atom with one or more dissimilar atoms, kations with anions, that we can deduce the color of the atoms themselves. At the outset, the following proposition must be established, namely, that in any colored inorganic compound in solution the color belongs essentially to the metallic atom, whether it exists in a free state as an ion or combined with a dissimilar atom or atoms to form a molecule. In other words, the color does not belong to the ion with exclusion of the molecule, nor to the molecule with exclusion of the ion. Color when it appears is the essential property of the atom, possessed by it in the free state and carried by it into any electrolyte which it forms. For proof of this the author cites the experiments of Glan and of Ewan, using copper sulphate solutions of different dilutions and noting the absorption. This was found to be the same when the light passed successively through distilled water and the solution and when it passed through the two mixed together. Since the dissociation was twice as great in the second case, it is clear that if the color depended on the ions only the absorption would have been more than doubled, while if it depended on the molecules only it would have been materially decreased. Hence it is obvious that the color belongs to the atom, whether it exists as an ion or whether by union with a dissimilar ion it forms part of an electrolyte. As criteria for determining the colors of ions the author uses the following: (1) When an electrolyte gives a dilute solution in water which is colorless, both the kation and the anion are colorless. Thus, since lithium bromide solution is without color, the ions of both elements are also colorless. So, too, since no relation exists between the color of an atom and that of its molecule, we find that the two colorless bromine atoms form the intensely colored element bromine. (2) When an electrolyte gives a colored dilute solution in water the constitution of the anion must first be considered. If it is a single atom, then the color of the solution belongs entirely to the kation, since all elementary anions are colorless. (3) Even when the anions is composite, conclusions may be drawn. In the case of sulphates, for example, whose anion is composite, colored dilute solutions likewise owe their color to the kation. These criteria afford the means of deciding upon the color of the entire series of elementary atoms. For the present purpose, however, it is not necessary to inquire what that color is, but only whether color is present or not.

Upon the results thus obtained the author founds, first, a new classification of the elements based on more correct principles than those previously made use of, and, second, a proof that the color or non-color of an element is a function of its atomic weight. Considering the elements numerically, it appears (1) that those whose atomic weights are less than 47 have colorless ions only; (2) that colored ions suddenly commence with titanium (48) and form an unbroken series of eight elements up to copper (63.4); (3) that a series of nine metals follows having colorless ions only, beginning with zinc (64.9) and ending with yttrium (92.5); (4) that next comes six metals with colored ions extending from niobium (94) to silver (107.7); (5) that these are followed by nine metals having colorless ions, from cadmium (111.6) to lanthanum (139); (6) that next come ten metals having colored ions, from cerium (142) to gold (196 2); (7) that the six remaining metals are alternately colorless and colored, mercury (199.8) being colorless, thallium (203.6) colored, lead (206.4) colorless, bismuth (210) colored, thorium (234.?) colorless, and, finally, uranium (240) colored. From the conception of the all-important nature of the color of the atom, while that of the element is of little significance, the author draws several interesting conclusions: First, that the well-known Periodic law must be rejected as based on erroneous principles; and, second, that *no element having ions colored at all valencies can belong to the same natural group with elements having colorless ions only.* This law, which he calls the Law of Color, is rigorous and fundamental; rigorous, because it admits of no exception; fundamental, because it divides elements into two chief divisions, with strongly marked differences.

As has been shown, the mass of the elements may be separated into two great divisions, those with ions always colorless and those with ions always colored. These two groups are always distinct, the elements in one having no relation with those in the other—*i. e.*, we never find in natural groups elements belonging to more than one of these two great divisions. If we arrange in the first division the elements whose ions are always colorless, placing them in numerical order in vertical columns of nine each, and then if we read the horizontal lines, we shall find that "*the entire class of elements with colorless ions is divided into nine great groups*," all absolutely natural, each element accurately fitting in its proper place. The table thus made includes all those elements whose ions function as anions with part of the kations. If the series of elements having all their ions colored be arranged in a second similar division, they fall into four series, the members of which have their atomic weights immediately following each other in unbroken succession. Intermediate between these two divisions is a small class of eleven transitionals, having ions which at some valencies are colored and at others colorless. The relations of this peculiar group are chiefly with the elements having colorless atoms only. With those having atoms always colored their relations are slight; but they have been given a place in the table with the latter to emphasize the fact "that in the entire series of elements *there is not a single case in which an element having atoms always colorless appears in the regular numerical series between a transitional element and one with atoms always colored. Also that there is not a single case in which an element with atoms always colored appears in the numerical series between a transitional element and one having colorless atoms only.*" This mixed group contains elements whose atoms function as kations only.

The paper concludes with a discussion of the periodicity of the law of color, illustrated by a plate, commencing with hydrogen and showing a double series of eighteen elements, with increasing atomic weights, all having colorless ions only. Approaching the first of the colored groups—*i. e.*, the iron group—we find the transitional elements titanium and vanadium, which have both colorless and colored ions, the former uniting them to the preceding and the latter to the following series. This alternation is continued through the list of the elements, showing that with atomic weights from 1 to 47, from 65 to 90, and from 112 to 139 their atoms are colorless; from 52 to 59, from 103 to 106, from 145 to 169, and from 192 to 196 the atoms are always colored. Elements whose place in the numerical series falls between these periods have both colored and colorless atoms. The six remaining metals have colorless and colored atoms alternately. Evidently the conclusion drawn by the author from these facts, "that the color of the elementary atoms is to a large extent a *function of their atomic weights*," is fully justified.

In his second paper, read before the National Academy the following year, Mr. Lea considers more in detail certain consequences of his general theory. The law of the interaction of ions he states thus: "If a colored substance be formed by the union of a colorless kation with a colorless anion, the color belongs to the molecule only. The colorless ions have so modified each other's vibration periods that selective absorption is exercised. As soon therefore as the molecule is divided into ions the color must disappear; consequently if we find a solvent, which, like water, is capable of separating the ions, the resulting solution when dilute must be colorless, no matter how intense the color of the compound." The truth of this law he experimentally tested, and found the results confirmatory without exception. With regard to the combinations of ions, he states as follows: A, two or more similar colorless ions may unite to form a colored elementary molecule; B, two or more similar ions, colored, may unite to form a colorless (or white) molecule or polymer; C, two or more similar colored ions may unite to form a molecule of a wholly different color; D, two or more dissimilar colorless ions may unite to form a colored molecule. No ion, and therefore no atom, is black, but is always transparent to some portion or portions of the visible rays; atoms and ions differing absolutely in this respect from molecules. In considering the theory of the action of acid indicators he maintains that dissociation has no essential connection with their reactions. The fact simply is that by combining with alkalies these substances either have their color much intensified or change it altogether. From the results of his color investigations Mr. Lea draws the following general conclusions: (1) When highly colored inorganic substances are composed of colorless ions, then if these substances can be brought into solution as electrolytes, the color wholly disappears. (2) The union of ions, colored and colorless, gives rise to the most surprising changes of color. (3) The change of color of an acid indicator placed in contact with an alkali in no way depends upon dissociation. (4) Selective absorption of the visual rays by an element can never constitute a basis for classification, but the relation of ions to the visual rays leads to a classification which is in absolute harmony with the chemical characteristics of the elements. (5) While there is good reason for believing that in solution the ions are separated so as no longer to affect each other's vibrations, it is also certain that they remain within each other's range of influence, so that they cannot be considered as free.

If I have been at all successful in presenting the salient points of the scientific life of Matthew Carey Lea, it will be clearly evident that he was a most industrious worker in the profounder regions of his favorite science. He was not only an observer of the acutest type, but he was at the same time thoroughly acquainted with the literature of chemistry, both past and present; so that he was readily able to appreciate to the full the real significance of the observations which he made and to use these observations to the best advantage. His power of work was well nigh unequaled and he was ever indefatigable in experimental research. Moreover, his memoirs greatly impress the reader with his exceptional candor. He was always conscientiously critical concerning himself when at work in his laboratory. The skill with which he planned and executed his experiments is equaled only by the intense love of truth which he showed in his criticism of them. He never expressed an opinion without having the facts in hand on which to base it, and in his papers he always gives the fullest and clearest details of his methods of work. In all the multifarious directions in which he studied and experimented he was ever most careful to treat those with whom he differed with the utmost courtesy. No one could differ from him on any matter without seriously questioning his own position on the subject in dispute.

Obviously this unremitting labor and this earnestness and honesty of purpose could not fail of its reward. He became an acknowledged authority on all the subjects he had sought to make his own. His opinion was recognized as the final opinion in photographic chemistry, both in this country and in Europe. He was everywhere regarded as the pioneer investigator in the more scientific realms of the art, and his studies on such subjects as the preparation of collodio-bromide emulsions, on the chemistry of developing agents, on the influence of color on the reduction by light of silver salts, especially the haloids, and particularly his work on the remarkable tendency of these substances to form colored compounds practically of all possible hues, and so foreshadowing the success of heliochromy in a not remote future—these studies must ever be considered the most valuable contributions to the science of photography made during the last quarter of a century in which he lived. His remarkable acuteness, for example, in suggesting the existence of what he so happily characterized as photosalts is equaled only by the analytical ability with which, sometimes by direct and sometimes by indirect methods, he finally fully established them. The possibility of hemisalts as he called them—salts which in the free state are extremely difficult, if not impossible, to isolate, but which readily form well-marked and very stable compounds with their normal or mono-allies—at first only a shrewd conjecture, became in his skillful hands an accomplished scientific fact. And his convincing proof that ordinary silver chloride is capable of combining with silver subchloride to form the photochloride of silver, and that this is the substance which constitutes the latent photographic image, is paralleled only by the experimental evidence he furnished that the action of light is not necessary to its production, but that it may be made by a variety of purely chemical and even mechanical processes.

Besides his scientific work, Mr. Lea was devoted to literature and the classics. In fact, he was an exceptionally good linguist, and therefore was constantly familiar with all that was best in the European languages.

In early manhood he met with an accident in his laboratory, which seriously injured one of his eyes, and which eventually required its removal. He was able, however, to keep up his interest in the current work of foreign literary scholars through the devotion of his wife, who for many years read to him constantly.

Although he was naturally retiring in his disposition and, owing, no doubt, to his continued ill health, lived the life almost of a recluse, yet his intimate personal friends bear willing testimony to his brilliance as a conversationalist. He belonged only to a few scientific organizations. In 1848 he connected himself with the Franklin Institute, taking a special interest in the Chemical Section. In 1895 he was elected a member of the National Academy of Sciences.

On the 14th of July, 1852, Mr. Lea married his cousin, Elizabeth Lea Jaudon, at that time the widow of William Woodhouse Bakewell, of New Orleans. She died on the 19th of March, 1881, leaving an only son, George Henry Lea, who is still living. His second wife was Eva Lovering, the daughter of Professor Joseph Lovering, of Cambridge, Mass., by whom he had no children.

Toward the close of his life he was seriously troubled with an affection of the prostate gland, causing intense suffering, and finally requiring an operation. Though apparently successful at first, his strength at his advanced age was not equal to the strain put upon it, and he died at his residence, at Chestnut Hill, on the 15th of March, 1897, in the seventy-fourth year of his age.

His large and valuable collection of books and apparatus was bequeathed to the Franklin Institute, together with a fund in perpetuity for the purchase of books and journals. Throughout his life he was a constant donor to charities of many kinds, a goodly number of the most prominent charitable organizations in his native city being handsomely remembered in his will.

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With kindest regards,

Faithfully yours,

Francis A. Walker

BIOGRAPHICAL MEMOIR

OF

FRANCIS AMASA WALKER.

1840-1897.

BY

JOHN S. BILLINGS.

READ BEFORE THE NATIONAL ACADEMY OF SCIENCES,
APRIL 17, 1902.

BIOGRAPHICAL MEMOIR OF FRANCIS AMASA WALKER.

Francis Amasa Walker, member and Vice-President of the National Academy of Sciences, was born in Boston, Mass., July 2, 1840, and died in the same city January 5, 1897. He was a son of Amasa Walker, the well-known teacher and writer, whose studies in political economy had great influence in directing his own line of work. He graduated at Amherst in 1860, and studied law in the office of Devens and Hoar, of Worcester, Mass., until the commencement of the civil war, when he enlisted as sergeant major in the Fifteenth Massachusetts, under the command of Colonel Devens.

He soon received a commission as captain and was rapidly promoted through the several grades to that of brigadier general, serving on the staffs of Generals Couch, Humphreys, Warren, and Hancock, being long connected with the latter, and the author of the History of the Second Army Corps and of the Life of General Hancock. He was severely wounded in 1863, and was captured in 1864 and confined for a time in Libby Prison. After the war he was on the editorial staff of the *Springfield Republican* in 1868; was Chief of the Bureau of Statistics in Washington from January, 1869, to February, 1870, and then became Superintendent of the Census. In 1872 he accepted the chair of political economy in the Sheffield Scientific School at New Haven; in 1879 he became Superintendent of the Census, and in 1881 he accepted the presidency of the Massachusetts Institute of Technology, which he held until his death. He was Commissioner of Indian Affairs in 1871, Chief of the Bureau of Awards of the Philadelphia Exposition in 1876, United States Commissioner to the International Monetary Conference at Paris in 1878, President of the American Statistical Association in 1882, and President of the American Economic Association in 1886. His career was a brilliant one in many fields of labor, and in most of them it was his peculiar ability as an organizer and administrator which gave him preëminence. He was a born leader—bold, frank, sincere, and entirely devoted to his work—

and in the public offices which he held and as chief of a great educational institution his subordinates and assistants became, almost without exception, his loyal and devoted friends. He was elected a member of the National Academy of Sciences in 1878, as being the most distinguished American representative of the scientific side of economics and statistical methods. His most important contributions to the theories of economics were his " Law of Wages " and his " Law of Profits." In his work on " The Wages Question," New York, 1876, he opposed the then prevailing theory that wages were paid out of what was called " a wages fund," taking the ground that while wages may be, and often are, paid out of capital in advance of the product, yet that they are ultimately paid from the product, and must in the long run be less than the product by enough to give the capitalist his due returns and the employer his living profits. This work was largely critical, being destructive rather than constructive, but it has had great influence upon the modern teaching on that subject. Subsequently he developed the theory that rent, interest, and profit depend upon certain fixed laws, and that whatever does not belong by these laws to rent, interest, and profit belongs to the laborer. This theory is not generally accepted by recent writers on the subject, although they admit that it accords with recent facts which they would explain in another way. It is not my purpose in this brief sketch either to set forth General Walker's economic views or to criticise them, and I will only say that while his theories on bimetallism are not generally accepted by recent writers on the subject, they were by no means those of the so-called " Silver Party," and that he strongly disapproved of the legislative measures which were proposed by this party in 1896, at the time when his work on international bimetallism was published.

A characteristic feature of General Walker's views on questions in economics and political science is that they take into account the emotional and altruistic side of human nature, as well as the purely intellectual and selfish side. The ethical relations of men in different countries at different times, and under different conditions of popular opinion as to the moral obligations of men of different classes, occupations, and interests had much weight with him in the formation of his opinions upon economic questions, and abstract propositions derived from the mathematics of exchange did not entirely govern his decisions as to what was the wise and right thing to do in a particular concrete case. In his own words, the political economist should take into account certain motives which influence men in respect to wealth, such as "love of country, love of home, love of friends, mutual sympathy among members of the same class, * * * good will between landlord and tenant, between employer and employed, the power of custom and tradition, the force of inertia, ignorance, and superstition."

He accepted the statement of Lightwood that "the object of law is to regulate the relations existing between men in such a way as to satisfy the sense of right existing in the community."* The laws of theoretical economics have been compared to the mathematical formula for the course of a cannon ball. This formula is valuable, but in its practical application the resistance of the air, the direction and force of the wind, and slight peculiarities in the gun itself must be taken into account if the mark is to be hit.

General Walker was not a sentimentalist or a socialist, but he was a warm-hearted man, full of vitality and sympathy, which influenced his writings and made them understandable by, and interesting to, a great mass of people not familiar with the technical details of so-called scientific economics.

As a statistician, his reputation rests mainly on his work in connection with the United States censuses of 1870 and 1880. He reorganized the methods of the census and broadened its scope, so that its published results have become of great importance in statistical literature, incomplete and imperfect as some of them undoubtedly are. His object was to give a comprehensive view of this country at the end of the first century of its existence so far as this could be done by statistical methods, and in this he was fairly successful. He did not propose that this should be repeated every ten years, but advised that a permanent Census Bureau should be established to carry on the work on more limited and strictly scientific lines. His successors at the head of the Census Bureau have worked along the lines laid down by him, and have also advocated the establishment of the

*The Nature of Positive Law, by John M. Lightwood, p. 26, London, 1883.

permanent Census Bureau, which has recently become an accomplished fact.

Of his work as an educator it is unnecessary to speak here. Its record will be found in the history of the Massachusetts Institute of Technology and in the paper by H. W. Tyler on "The Educational Work of Francis A. Walker," published in the *Educational Review*, 1897, vol. XIV, pp. 55–70.

A good bibliography of General Walker's writings is given by J. Laurence Laughlin in the *Journal of Political Economy*, vol. 5, March, 1897, pp. 232–236, and a copy of this is appended.

The most important part of his work is not so much in his writings, interesting and valuable as many of them are, as in the influence which he exerted upon his assistants, associates, and pupils, and which appears in the character and amount of the work which they have been and are still doing. No American of his time was more widely known both in this country and abroad. He was the recipient of many academic and society honors, and his sudden death while in the full tide of successful effort was felt as a calamity, not only by his numerous personal friends, but by many who knew him only indirectly through his books and essays.

In the National Academy the place which he left is still vacant, and it will be long before it will be filled by one who will equal him in his knowledge of the wide field covered by his activity, and who will also be as perfect a gentleman and as agreeable an associate.

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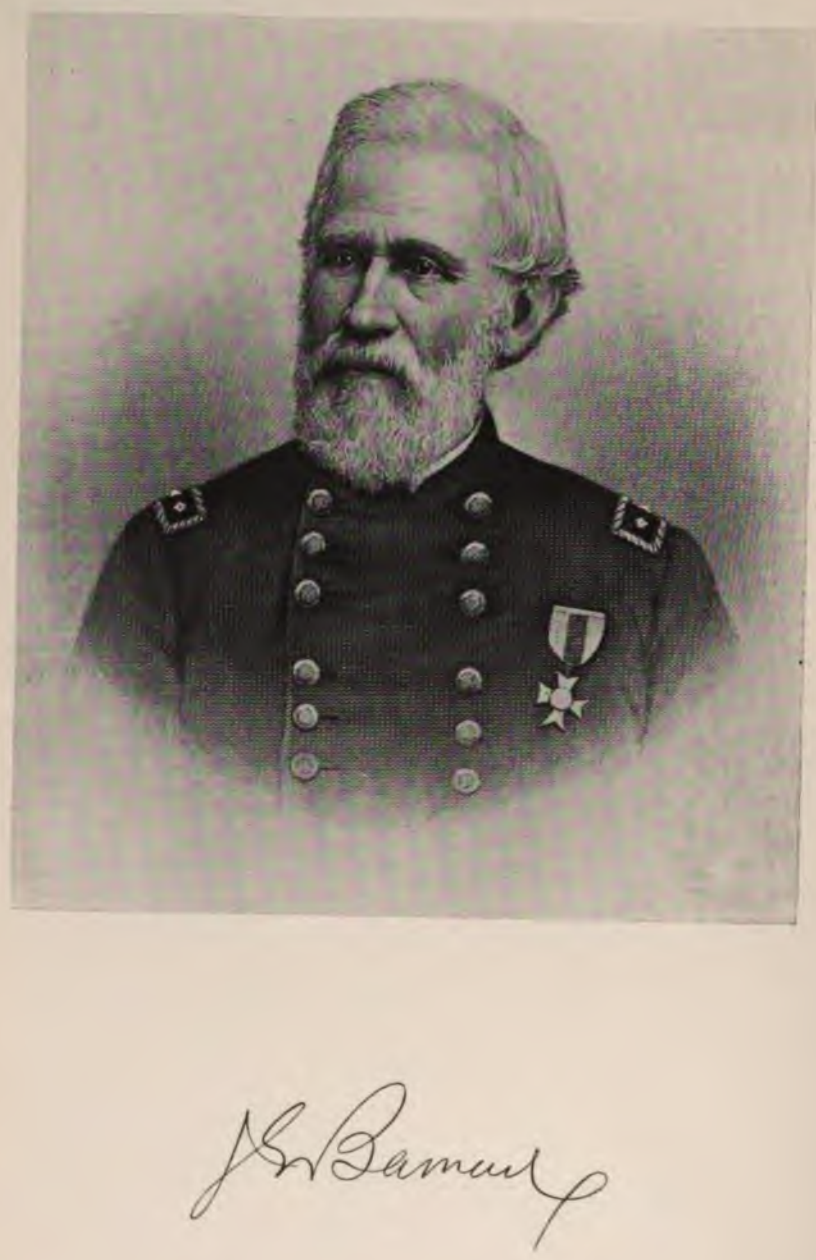

BIOGRAPHICAL MEMOIR

OF

JOHN GROSS BARNARD.

1815–1882.

BY

HENRY L. ABBOT.

READ BEFORE THE NATIONAL ACADEMY OF SCIENCES,
APRIL 17, 1902.

BIOGRAPHICAL MEMOIR OF JOHN GROSS BARNARD.

The act of March 3, 1863, incorporating the National Academy of Sciences, contains the name of General JOHN G. BARNARD as one of the original fifty members, and his interest in its object and development ended only with his life.

He was born in Sheffield, Mass., on May 19, 1815, being the second son of Robert Foster and Augusta Porter Barnard. His father, the son of Dr. Sylvanus and Sarah Gross Barnard, was a lawyer of marked ability, known beyond the circle in which he lived, although always leading the quiet life of a small town. General Barnard's mother traced her descent from a somewhat remarkable old New England family. Among the early settlers of the country, John Porter and his wife, Rose, settled at Windsor, Conn., which he represented in the legislature in 1646. In the fourth generation Colonel Joshua Porter married Abigail Buel, the daughter of Peter Buel, whose wife was the widow of Noah Grant, one of the ancestors of General U. S. Grant. Thus it happens that the great-grandmother of General Grant and of General Barnard was one and the same person.

The boy spent the first twelve years of his life at Sheffield, attending the village school, and was then sent to begin his more advanced studies under his brother, our late colleague, President Barnard, who, after graduating at Yale College, was then teaching school in Hartford. A year later his great-uncle, General Peter B. Porter (for whom Fort Porter, at Black Rock, Buffalo, is named), who was then Secretary of War, offered the boy an appointment at West Point. This was gladly accepted, and he entered in 1829, having just passed his fourteenth birthday and being probably the youngest pupil ever admitted.

He was graduated at the end of the four years' course with second rank in a class of 43 members, several of whom attained distinction in their subsequent careers. He was assigned to the corps of engineers as brevet second lieutenant on July 1, 1833, and, passing through all intermediate grades, became colonel on December 28, 1865, having received five brevets for distinguished services in the Mexican and the civil wars. He was retired on

January 2, 1881, and died on May 14, 1882. Such in brief is the outline of a career which exerted no small influence upon the current events, both civil and military, of the times in which he lived.

In the civil branches of his profession General Barnard's services covered works of construction and of internal improvement extending from the Great Lakes to the Gulf of Mexico and from the Atlantic to the Pacific coast. In this wide area few important engineering problems engaged the attention of the government in which his advice was not officially demanded, either individually or as a member of special boards and commissions. His professional duties were not even restricted to the limits of the United States. During the war with Mexico he superintended the construction of defenses at Tampico and made surveys about the City of Mexico, and in 1850 he was named by the President chief of a scientific commission for the survey of the Isthmus of Tehuantepec with a view to establish a route of commerce and travel to our newly acquired Pacific possessions. The report drawn up by J. J. Williams, in 1852, gives the first full account ever published of that isthmus. The exposures incident to this service in the tropics affected his health so seriously that he never entirely recovered. In later life he was sent to Europe twice as a member of commissions to collect information needed by the government.

General Barnard's military services during the civil war were conspicuous. At its very outbreak he initiated, as chief engineer of the Department of Washington, field works for the defense of the city. In the Bull Run campaign he served as chief engineer on the staff of General McDowell; and the details of the general plan to turn the enemy's left were established upon his personal reconnaissances, made on the day before the battle, of the route by Studley Springs. At the organization of the Army of the Potomac, in August, 1861, he became its chief engineer, and after greatly extending the defenses of the city he accompanied General McClellan in the spring to the peninsula, and served as his chief engineer during the entire campaign. At the siege of Yorktown he commanded all the engineer troops, and directed the location and construction of the batteries and approaches. On the Chickahominy he was charged with the construction of the bridges and batteries, and was often consulted as to the position and movements of the troops. After the occupation of Harrison's Landing, contrary to the views of General McClellan, he favored the withdrawal of the army to Washington. On the 16th of August he was individually recalled to that city, and was placed in command of the fortifications, including the troops assigned to their defense; but on the 2d of September he relinguished the latter, not having a rank commensurate with the duty. His commission as brigadier general of volunteers dated from September 23, 1861, while the troops which had become available for defending the city were often commanded by officers of higher rank. He, however, retained the charge of the defenses until their essential completion early in 1864, and was often called upon for reports and advice as to general plans, such as operations against the chief ports of the enemy, the defense of Harper's Ferry, of Pittsburg, of the lake shore, against raids from Confederates in Canada, and also as to important naval problems. In January, 1864, he applied for duty in the field, and on the 5th of June was assigned to General Grant's staff as chief engineer of the armies in the field. He served in that capacity until the surrender of Lee's army, on April 9, 1865, taking an active part in the operations in Virginia. As his office was finally organized, weekly reports of the chief engineers of the two armies, monthly reports of materials received and expended, requisitions for engineer supplies, etc., were submitted to him at General Grant's headquarters. He also devoted much time to careful inspection of the extended lines.

In recognition of his services in the civil war General Barnard received the brevets of colonel, brigadier general, and major general in the regular army, and of major general in the volunteers. He was also named in the act of March 3, 1865, as one of the one hundred corporators to establish a military asylum for disabled volunteers.

Although present in many important battles in the civil war, and assisting by his counsels in the decision of many important military problems affecting naval operations, as well as those on land, it is upon his services as chief engineer of the defenses of Washington that are based his most enduring claims to remembrance in that crisis of our national history. From a military point of view, the geographical and topographical location of the capital was unfortunate. It lay within the region where the most important struggles must have place, and it occupied a plain surrounded by commanding heights throughout the greater part of whose circumference an attack might be apprehended if the covering army should experience a serious check. Its loss, even if temporary, must entail disastrous consequences, not only directly upon the conduct of the war, but also indirectly upon our foreign relations, then not always of the most friendly character. These conditions were perceived, but not fully appreciated, in the blind confidence prevailing before the first Manassas campaign; but, after that repulse of the army, the necessity of putting Washington in a condition to be defended by a moderate garrison before the Army of the Potomac could move from its immediate front was seen by all persons of intelligence. Before the first advance, a few field works in the nature of têtes-de-pont had been thrown up to cover the Aqueduct bridge, the Long bridge, and Alexandria, and it is doubtless due to their presence and imperfectly known development that no demonstration was made by the Confederates in front of Washington at the time when demoralization was at its height after the battle of Bull Run. It was fortunate that the duty of extending and perfecting these preliminary works of defense devolved on an engineer so competent to appreciate the problem as was General Barnard. The works of Torres Vedras furnished the best example, but the conditions were so different that a master mind was required for a judicious application of the principles involved. Something more than ordinary field works, but less exacting in time of construction than usual works of permanent defense, was called for; and General Barnard, par excellence, was the man for the occasion. His own monograph, published in 1871 as No. 20 of the Professional Papers of the Corps of Engineers, fully details the semi-permanent system adopted, and will long remain a standard authority on this novel application of the principles of fortification. When completed, the lines enclosed Washington by a cordon of works aggregating 37 miles in length, with 68 inclosed forts and batteries having a perimeter of about 13 miles, actually mounting 807 guns and 98 mortars, with many other emplacements, together with 20 miles of rifle trenches, three block-houses, and 32 miles of military roads, in addition to those of the District. The utility of these fortifications during the civil war can hardly be better set forth than in the modest language of their designer:

"When the Army of the Potomac, in 1862, was beaten in the field and to some extent demoralized and disorganized, it fell back on the defenses, where it rested in security; a very few days of respite, the arrival of reinforcements, and a change in the commander enabling it to take the field again offensively.

"When Early marched on Washington, in 1864, the defenses had been stripped of the disciplined and instructed artillery regiments (numbering about 18,000 men) which had constituted their garrison, and their places supplied by newly raised 100-days' regiments (Ohio National Guards), insufficient in numbers and quite uninstructed. Under such circumstances much anxiety was felt on the approach of Early's veterans, flushed with recent success, inspired by the very audacity of their enterprise, and incited by the prize before their eyes. Yet, inadequately manned as they were, the fortifications compelled at least a concentration and an arraying of force on the part of the assailants, and thus gave time for the arrival of succor."

Soon after the close of the war General Barnard was made president of the permanent Board of Engineers for Fortifications and River and Harbor Improvements, a position which he held until his retirement from active service, in January, 1881. The epoch was one of radical transition in coast defense and of vast extension in our works of internal improvement. Our system of masonry coast defenses, to the elaboration of which our former colleague, General Totten, had devoted his life, and which General Barnard has so admirably set forth in his biographical memoir, read before the Academy on January 6, 1866, had been rendered antiquated by the enormous increase in the size and efficiency of heavy guns and by the success in the efforts to mount them on ships protected by armor against shell fire. Our coasts, which had been furnished with fortifications superior to any existing in Europe and in a good state of progress toward completion, were found to be open to attack by a modern fleet of armored battle ships. A new type of shipping had appeared just before and during the civil war, and a new system of coast defense would ultimately be demanded. Pending the necessary studies to determine its character, the existing works must be modified and strengthened to meet immediate needs. The new problem of ordnance and armor was then occupying the attention of the ablest engineers abroad, and General Barnard brought to the study a mind ripened by practical experience in war, a thorough understanding of the fundamental principles involved, and a technical knowledge of the new developments. Experiments on a large scale were at once inaugurated at Fort Monroe and Fort Delaware by the engineer department, and General Barnard, with able coadjutors, was sent to Europe to study the new problems in the light of the most recent investigations there. We have now no occasion to regret either false conclusions or unwise recommendations by the board of which he was so long the president and leading member. During these years he also served as a member of various special boards charged with investigations looking to the improvement of navigation in certain western rivers and at the mouth of the Mississippi, and for a long time was a member of the Light-house Board.

The degree of A. M. was conferred upon General Barnard by the University of Alabama in 1838, and that of LL.D. by Yale College in 1864. He was a member of the American Institute of Architects, and an honorary member of the American Society of Civil Engineers.

Throughout life General Barnard was an untiring student, and he wrote with facility and to the point. Even at the Military Academy he had shown uncommon mathematical ability, and he subsequently carried original investigations in this direction much beyond the limits usually attained by men of so busy a professional career. His papers on the gyroscope and kindred problems, published in *Silliman's Journal* before the civil war, are examples in point. His writings on technical engineering subjects, both civil and military, were voluminous, and many of them will long remain authorities on the subjects of which they treat. Among them may be mentioned: Notes on Seacoast Defence (1861); Reports of Engineer and Artillery Operations of the Army of the Potomac, prepared with General Barry (1863); Report on the Defences of Washington (1871); Report on the Fabrication of Iron for Defensive Purposes, prepared with Generals Wright and Michie (1871); North Sea Canal of Holland and Improvement of Navigation from Rotterdam to the Sea (1872); Problems of Rotary Motion Presented by the Gyroscope, the Precession of Equinoxes, and the Pendulum (1872).

It is, however, in Johnson's Universal Cyclopædia, published in 1874–1877, that the versatility and precision of his mental culture are best shown. He found time to act as one of the associate editors in its preparation, and over ninety scientific and other articles, some of them almost treatises, are from his pen. Among them may be named: Aeronautics; Breakwater; Bridge; Bull Run, Battle of; Calculus; Gyroscope; Harbor; Imaginaries; Laplace's Coefficients; Light-house Construction; Rotation; Tehuantepec; Variations, Calculus of; and Tides, Theories of. Few engineers have been more profoundly versed in their profession or more able to give reasons for their convictions.

In early life, when stationed in New Orleans, he married Miss Jane Elizabeth Brand, daughter of William Brand and sister of the Rev. William F. Brand, of Maryland, one of the noted clergymen of that state. Four children were born to them, of whom one son survives. In 1860 he married Anna E., daughter of Major Henry Hall, of Harford county, Maryland, whose ancestor emigrated with Lord Baltimore; and their three children are all living.

In his personal characteristics General Barnard was a thoughtful, self-contained, and earnest soldier. Under fire he seemed to have no sense of exposure, and in his frequent reconnaissances he was wont to push aside advanced pickets attempting to advise him as to the position of the enemy's sharp-shooters, apparently trusting more to his own intuitions than to their local knowledge. His inherited deafness rendered social intercourse somewhat difficult, and to those who did not know him intimately this circumstance perhaps conveyed the idea of coldness and formality; but such was far from being his nature. As his aide-de-camp during the Peninsular campaign, I often saw evidences of the warm interest he took in the success of the many young officers serving under his orders and of his cordial appreciation of good work done by them. His brother, our late colleague, being at the south at the outbreak of hostilities, had experienced no little difficulty in crossing the line or communicating with his family, and when he suddenly appeared unannounced in the general's tent one evening, the fraternal embrace which followed proved that they both concealed warm hearts under a dignified exterior. He had a keen sense of humor and a passionate love of music. Indeed, he composed many pieces—among others, a *Te Deum* that still survives.

General Barnard was nominated by the President, on the death of General Totten, to succeed him as brigadier general and chief of engineers, April 22, 1864; but the nomination was withdrawn, at the request of General Barnard, before any action was taken by the Senate. He never, to my knowledge, made public his reasons for this request; but the facts suggest the fair inference not only that President Lincoln highly appreciated the merit of General Barnard's services, but also that the latter's sense of justice to a superior in rank in the corps of engineers forbade him to take advantage of this appreciation. Such self-sacrifice is not common, either in or outside of the army.

The estimation in which General Barnard was held in the corps of engineers is well expressed in the concluding paragraphs of the order of General Wright announcing his death:

"A service of nearly fifty years in the corps of engineers has been closed by the death of one of the most prominent of its members.

"Of greatly varied intellectual capacity, of a very high order of scientific attainments, considerate and cautious, ripe in experience, sound in judgment, General Barnard has executed the important duties with which he has been charged during his long and useful life with conscientious care and regard for the public interests and with an enthusiastic devotion to his profession. His corps, the army, and the country are his debtors.

"Modest and retiring in disposition, considerate and courteous, warm in his sympathies and affections, our deceased associate will be missed as few are missed, and his name, which will be held as one of the foremost names of the corps of engineers, will be cherished with peculiar love and affection by his brother officers."

LIST OF THE MORE IMPORTANT PAPERS PUBLISHED BY GENERAL J. C. BARNARD.

Phenomena of the Gyroscope analytically examined. 1858.

Dangers and Defences of New York City. 1859.

Notes on Seacoast Defence. 1861.

The C. S. A. and the Battle of Bull Run. 1862.

Reports of the Engineer and Artillery Operations of the Army of the Potomac from its Organization to the Close of the Peninsular Campaign. (Jointly with General Barry.) 1863.

Eulogy on the late Major-General Joseph G. Totten, late Chief Engineer, U. S. A. 1866.

Report on the Defences of Washington. (P. P. Corps of Engrs., No. 20.)

Fabrication of Iron for Defensive Purposes. (Jointly with General Wright and Colonel Michie.) (P. P. Corps of Engrs., No. 21, and supplement.)

Report on the North Sea Canal of Holland. (P. P. Corps of Engrs., No. 22.)

Problems of Rotary Motion presented by the Gyroscope, the Precession of the Equinoxes, and the Pendulum. Smithsonian Contributions to Knowledge, vol. XIX, 56 pages, 1872.

On the Internal Structure of the Earth considered as Affecting the Phenomena of Precession and Nutation, being the Third of the Problems of Rotary Motion. Smithsonian Contributions to Knowledge, vol. XXIII, 19 pages, 1877.

Many articles in Johnson's Cyclopædia on scientific subjects, such as Bridge Building; Harbor, Jetty, and Light house construction; Calculus, Aeronautics, Imaginaries, Gyroscope, Theory of Tides, etc.

J. E. Keeler.

BIOGRAPHICAL MEMOIR

OF

JAMES EDWARD KEELER,

1857-1900.

BY

CHARLES S. HASTINGS.

READ BEFORE THE NATIONAL ACADEMY OF SCIENCES,
APRIL 23, 1903.

BIOGRAPHICAL MEMOIR OF JAMES EDWARD KEELER.

JAMES EDWARD KEELER was born in La Salle, Illinois, on September 10, 1857.* His father, from whom he inherited his extraordinary taste for mechanical pursuits, and to whom he also owed his early instruction in the use of tools and the design and construction of innumerable conveniences of daily life which proved so invaluable to him in later life, William F. Keeler, was an officer on the *Monitor* at the time of its famous engagement with the *Merrimac.* His mother was the daughter of Henry Dutton, formerly governor of Connecticut and dean of the Yale Law School. Through this inherited association with New Haven he possessed an acquaintance and relationship with several families prominent in the society of that city, which, happily for them and for himself, he had an opportunity to cultivate later in his life.

In 1869 Mr. Keeler removed with his family to Mayport, Florida, and here Edward, as he was always known to his intimate friends, commenced that extraordinary education which proved so perfectly adapted to his exceptional genius. Mr. Keeler found it necessary in a community where neighbors were few and the conditions of life were largely those of a new and unsettled country to do a large part of his work as architect and builder with his own hands, and in this work we are assured that his young son assisted with the keen interest and eminent success which rendered him so useful a few years later to those who could command his services as an assistant in scientific work.

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* Biographical notices of Mr. Keeler have been published by Professor W. W. Campbell, director of the Lick Observatory; by Professor G. E. Hale, director of the Yerkes Observatory, and by Chancellor J. A. Brashear, of the Western University of Pennsylvania. From these the present writer has drawn freely, and from the notice by the first-named writer the account of Mr. Keeler's activity as director of the Lick Observatory is taken without change, since no one else is so perfectly acquainted with this chapter of his life The list of the published writings of Mr. Keeler is taken from the twelfth volume of the Astrophysical Journal, and was also prepared by Director Campbell.

It was in the boy's eighteenth year that his interest in astronomy had grown to such a point that he could be content with nothing less than an observatory of his own. He purchased lenses for a two-inch telescope with money which he had earned by his own efforts and mounted them himself. His journal records that the lenses, ordered from a dealer in the north, came to his hands on November 29, 1875, and that he first directed the telescope to the stars on the 12th of the next month, when he saw the ring of Saturn for the first time. This marks the beginning of a career as an observer which was unhappily cut short just at a time when he may fairly be thought to have reached his highest and most fruitful degree of skill. It is worthy of remark that the surviving "Record of Observations made at the Mayport Observatory" contains beautifully colored sketches of Jupiter, Saturn, Venus, Mars, the Orion Nebula, and of the moon, which are the forerunners of the famous and unequaled drawings of the great planets which were made a dozen years later with the Lick telescope.

This youthful activity was, however, something more than the merely playful exercise of intellectual faculties such as is often exhibited by clever boys, and which, admirable in itself, is little likely to lead to a lifelong devotion to science or to permanent additions to the sum of human knowledge. In Keeler's case we do not have to look far for signs which mark a true and rare superiority, for in his records of observations we find data of a numerical character which imply an abstract interest in science at once separating him from those who can hardly hope to find more than a pastime in science. His next step, in 1877, separates him from the pure amateur, for the construction of a meridian instrument, which he made from a marine spyglass with the tools at command in his own home, marks a mind which demands much more from science than amusement. A description and sketch of this instrument by Keeler himself is published in the twelfth volume of the Publications of the Astronomical Society of the Pacific, and its perusal will go far to show the rare ingenuity of its maker as well, it seems to the writer, as the admirable fitness of the early education of the youth for the work which lay before him.

This was nearly the end of Keeler's work among such limited surroundings, for his talents were soon to be recognized by a wise and generous man, who proved his lifelong friend and adviser, and who procured for him an opportunity to exhibit to competent and appreciative men his rare qualities and promise. Mr. Charles H. Rockwell, of Tarrytown, New York, had invited a class of young ladies at a private school in Tarrytown to visit his well-equipped observatory to view some of the more interesting astronomical objects, in accordance with a generous custom. His attention was attracted by one of the young ladies remarking that Saturn was familiar to her from having seen it through her brother's telescope at home. The young lady was Keeler's sister, and her remark interested Mr. Rockwell to such a degree that he was led, not only to inform himself farther about the young man, but to secure for him an opportunity to pursue a university training in the north. Rarely has such interest been more wisely accorded or more nobly received. One of Keeler's most admirable traits, known to all his intimates, was his unassumed and constant affection for this older and generous friend.

Mr. Rockwell took young Keeler to the Sheffield Scientific School of Yale University in the first case in order to enter him there as a regular student, but he was advised to place him as a special student at the Johns Hopkins University, where the highly irregular training and preparation acquired by the candidate could be more readily adapted to the requirements, less rigidly fixed in the new university, for effective college training. It was here that the present writer became intimately acquainted with Keeler, and it is from this long intimacy that he will find something to write concerning his personal characteristics, after having reviewed his scientific history and work.

Mr. Keeler's university career was not only a successful one, but it was also an extremely happy one. He possessed the power of making friends among his older associates as well as with the men of his own age, and the college work, which he did well and with ease, afforded him the keenest pleasure. His interest in his chosen science never flagged, although it was never allowed to interfere with other duties which belonged to him as a candidate for an academic degree. Even at the end of his freshman year he had a delightful opportunity to occupy himself again with his favorite observations as a member of Professor Holden's expedition from the Naval Observatory which observed the total eclipse of July 29, 1878, at Central City, Colorado, and which counted Mr. Rockwell and the writer among its members. A sketch and description of the corona on this occasion constituted the first of Keeler's published papers.

In the spring of 1881 Professor Langley requested the Johns Hopkins University to recommend a suitable man for the place of assistant in the Allegheny Observatory. Of two candidates highly recommended Keeler was chosen because one of his instructors emphasized the fact that it was always easy to find just what he did not know, by which it was intended to describe in an epigrammatic way his remarkable intellectual candor. Indeed, this was one of the most constant and delightful characteristics which belonged to him to the end of his life. No one could be more modest and unassuming in all intellectual matters; so much so that only those who were long and intimately associated with him were likely to recognize his real mental superiority.

The years from 1881 to May of 1883 were spent at Allegheny as Professor Langley's assistant, interrupted only by the famous trip to Mt. Whitney, when his chief made his famous determination of the solar constant. During this period he not only acquired a highly cultivated skill in the use of Langley's delicate and difficult investigations in the domain of radiant energy, but he also made many friends, who added to his pleasure and efficiency in the years later when he became the director of the Allegheny Observatory. After a year spent in study at the Universities of Heidelberg and Berlin, at the latter of which he made an investigation of the absorption by carbon dioxide of radiant heat with the use of Langley's newly invented bolometer, he returned to Allegheny. Here he remained from June, 1884, to April, 1886, assisting Professor Langley in his famous researches on the radiation of the moon and on the infra-red portion of the solar spectrum.

Early in 1886, on Professor Holden's recommendation, Mr. Keeler was appointed assistant to the Lick trustees. He arrived at Mt. Hamilton on April 25, 1886, and immediately proceeded to establish the time service. The telegraph line to San José was perfected; the transit instruments, the clocks, and the sending and receiving apparatus at both ends of the line were installed. The signals were sent out on and after January 1, 1887—north to Portland, east to Ogden, and south to San Diego and El Paso. In addition to the time service, he assisted the trustees in installing the various instruments.

When the observatory was completed and transferred to the regents of the University of California, on June 1, 1888, Mr. Keeler was appointed astronomer, the original staff consisting of Astronomers Holden, Burnham, Schaeberle, Keeler, and Barnard and Assistant Astronomer Hill.

Professor Keeler was placed in charge of the spectroscopic work of the observatory. The large star spectroscope, constructed mainly from his designs, has no superior for visual observations. Of the many results obtained with this instrument, we may mention the observation of Saturn's rings and Uranus, with reference to their atmospheres; of the bright and dark lines in the spectra of γ Cassiopeiæ and β Lyræ; of the color curve of the 36-inch equatorial, and of the spectra of the Orion Nebula and thirteen planetary nebulæ.

His beautiful observations on the velocities in the line of sight of these fourteen nebulæ mark a distinct epoch in visual spectroscopy. His memoir on the subject took its place as a classic at once. The probable error of the final result for each nebula, based on the mean of several observations, is only $\pm$ 3.2 kilometers per second. Attention should be called to one extremely important fact established by these measures, viz., the velocities of the nebulæ in their motion through space are of the same order of magnitude as the velocities of the stars.

The recognition of the fact that a great refracting telescope is also a most powerful spectroscope for special classes of objects, by virtue of the chromatic aberration of the objective, is due to Professor Keeler. Among the first objects observed with the 36-inch equatorial were the planetary nebulæ and their stellar nuclei. The observers were struck with the fact that the focal length for a nebula is 0.4 inch longer than for its stellar nucleus, a discrepancy which Professor Keeler at once explained by recalling that the star's light is yellow, whereas that of the nebula is greenish-blue.

Astronomical readers will remember Keeler's splendid drawings of the planets Saturn, Jupiter, and Mars, made with the assistance of the 36-inch telescope during 1888–1890. His faithful and artistic drawings of Jupiter have no equal.

He was in charge of the very successful expedition sent by the Lick Observatory to Bartlett Springs, California, to observe the solar eclipse of January 1, 1889.

Professor Keeler resigned from the Lick Observatory staff on June 1, 1891, to succeed Professor Langley as director of the Allegheny Observatory and professor of astrophysics in the Western University of Pennsylvania. The Allegheny Observatory has perhaps the poorest location of any observatory in this country for spectroscopic work, but in spite of this disadvantage, Keeler's investigations continued and promoted the splendid reputation established for the observatory by his predecessor. He comprehended the possibilities and limitations of his situation and his means and adapted himself to them. His spectroscopic researches were largely confined to the orange, yellow, and green regions of the spectrum, since these would be less strongly affected by the smoky sky for which that vicinity is famous.

The Allegheny spectroscope, designed and constructed soon after his acceptance of the position, contained several valuable improvements. The use of three simple prisms in its dispersive train was a departure which has been followed with great advantage in many later instruments. With this instrument he made an extensive investigation of the Orion Nebula and the stars immersed in it, establishing the fact that the nebula and the stars are closely related in physical condition.* His beautiful observations of Saturn's rings, proving that they are a cluster of meteorites—myriads of little moons—have never been surpassed in interest in the entire astronomical field. These observations are so well known to every one interested in astronomy that a single sentence suffices. He proved spectrographically, using the Doppler-Fizeau principle, that every point in the ring system is moving with the velocity which a moon would have if situated at that distance from the planet. Professor Keeler's main piece of work at the Allegheny Observatory, on the spectra of the third (Secchi) type stars, remains unpublished, but the measures and reductions are left in an advanced stage.

The regents of the University of California appointed Professor Keeler to the position of director of the Lick Observatory on

* Simultaneous observations of the same object made at another observatory led to the same conclusion.

March 8, 1898. The ties which bound him and his family to Allegheny were difficult to sever, but the greater opportunities offered by the instruments and the atmospheric conditions at Mt. Hamilton decided him in favor of accepting the appointment. He entered upon his new duties on June 1, 1898.

Without making any rearrangement of the work of the staff, but affording them every possible encouragement to continue along the same lines, Professor Keeler arranged to devote his own observing time to the Crossley reflector. He recognized that the instrument was not in condition to produce satisfactory results. He made one change after another, overcoming one difficulty after another, until, on November 14, he secured an excellent negative of the Pleiades, and on November 16 a superb negative of the Orion Nebula. The enormous power of the reflector in nebular photography was established, and he entered upon the programme of photographing all the brighter nebulæ in Herschel's catalogue. More than half of the subjects on the programme have been completed. The observatory possesses a set of negatives of the principal nebulæ which is priceless and unequaled. These photographs have already led to many discoveries of prime importance, and they furnish a vast amout of material for future investigations of questions bearing especially upon the early stages of sidereal evolution. The photographs record, incidentally, great numbers of new nebulæ, as many as thirty-one on a single plate covering less than one square degree of the sky. A conservative estimate places the number within reach of the Crossley reflector at 120,000, of which only ten or fifteen thousand have thus far been discovered.

It has previously been supposed that the great majority of nebulæ are irregular and without form, and that only a few are spirals. Professor Keeler's photographs have recorded more spiral nebulæ than irregular ones. This discovery bears profoundly on the theories of the cosmogony, and must be considered as of the first order.

The honorary degree of Sc. D. was conferred upon Professor Keeler in 1893 by the University of California. He received the Rumford medal from the American Academy of Arts and Sciences in 1898, and the Henry Draper medal from the National Academy of Sciences in 1899. He was elected a member of the National Academy of Sciences at the April meeting in

1900. He was also an associate of the American Academy of Arts and Sciences, a fellow and foreign associate of the Royal Astronomical Society, a fellow of the American Association for the Advancement of Science, a member and officer of the Astronomical and Astrophysical Society of America, an honorary member of the Toronto Astronomical and Physical Society, the president of the Astronomical Society of the Pacific, a member of the Washington Academy of Sciences, and various other organizations.

It appears that Professor Keeler had long been a sufferer from a mild form of heart weakness; to run even fifteen steps caused him great physical distress. It is feared that on Mt. Hamilton he worked beyond his strength. He went away from the observatory on July 30, in the best of spirits and with no anxiety, to secure medical treatment and to spend a brief vacation in the northern part of the state. Increasing difficulty in breathing led him to seek skilled treatment in San Francisco on August 10. His dangerous condition was recognized on August 11, and on the 12th a stroke of apoplexy proved fatal.

When the dangerous weakness of his heart was discovered by the physicians, Professor Keeler's main regret was that he would have to leave Mt. Hamilton and its opportunities in order to live at a lower altitude. It is known that he had planned his work with the Crossley reflector far into the future. A small spectrograph which he was most anxious to employ on certain interesting spectra was completed on the day of his leaving the observatory.

Professor Keeler married Miss Cora S. Matthews at Oakley plantation, Louisiana, on June 16, 1881. He left two children.

To those who knew Keeler during the formative period of his life, either when a lad at Mayport or when more mature at the university, many delightful characteristics are inseparably connected with his memory. Perhaps those who were incapable of understanding his scientific aspirations were more impressed by his unfailing good humor, his gentleness and kindliness, and by a very exceptional sense of the humorous, which not only contributed greatly to his enjoyment of life, but gave him the appearance of being an eminently happy man. His interest in life was keen and rational. With perfect adaptability to the circumstances of the moment, he seemed to find not only sources of pleasure in them, but the kind of opportunities for intellectual occupation which is indispensable to minds of his type. The writer carries in vivid recollection his story of a trip from Mayport to the north in a small vessel, when he astonished the master by determining the place at sea by observations on stars at night, while the navigator had supposed that an observation of the sun was always necessary. To such a man children afforded endless delight, and doubtless many others can recall scenes where he utilized his extraordinary skill with the pencil as a means of entertaining them, when it was difficult to determine whose laughter was the more merry and infectious.

As a worker in science, his achievements were highly interesting. Although remarkably ingenious in devising expedients to meet his ends, he was never carried away by an admiration for his own creations, but was ever ready to reject any plan for a better one; in short, he always seemed to attain the very best expedient which inevitable conditions imposed. His work impressed the observer as so much like play that it was always a surprise to find that it generally proved an important contribution to science. Never hurried, never anxious, to watch him was a pleasure and to work with him was an inspiration. Apparently careless of scientific honors, such came to him in extraordinary abundance, and a longer life would surely have brought him fame to which we can set no probable limit.

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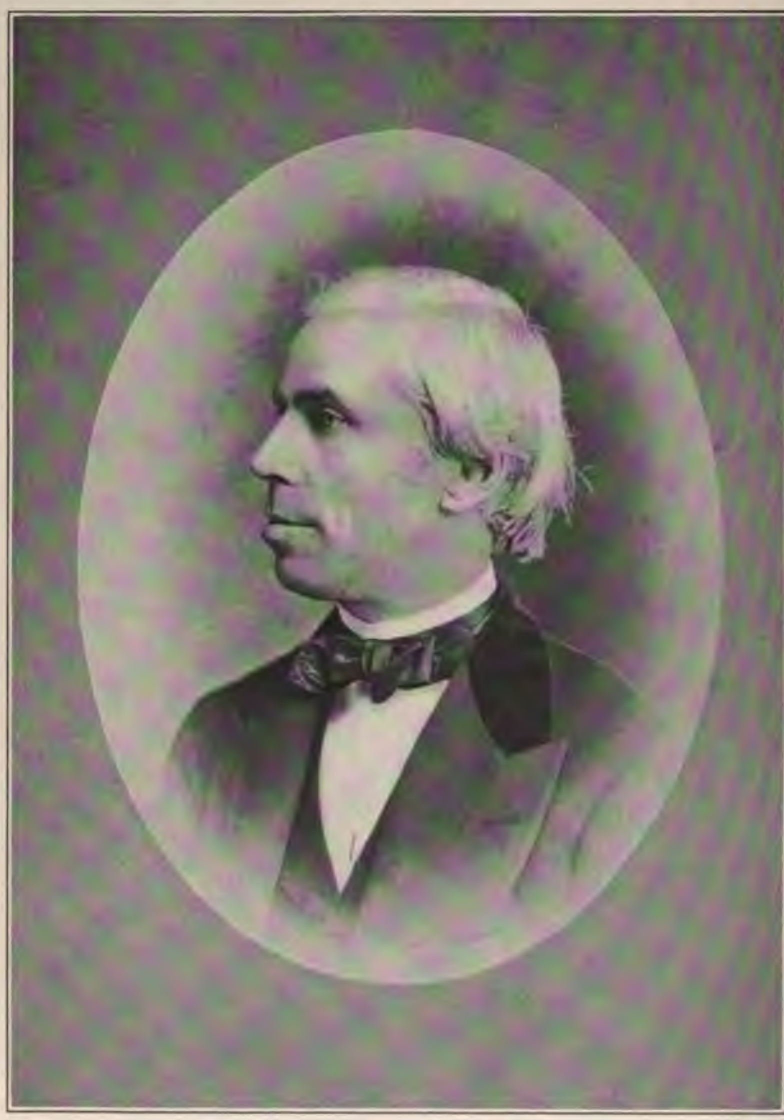

James Hadley

BIOGRAPHICAL MEMOIR

OF

JAMES HADLEY,

1821-1872.

BY

ARTHUR TWINING HADLEY.

READ BEFORE THE NATIONAL ACADEMY OF SCIENCES,
APRIL 21, 1904.

BIOGRAPHICAL MEMOIR OF JAMES HADLEY.

JAMES HADLEY was born in Fairfield, Herkimer County, New York, March 30, 1821.

The little town of Fairfield, lying on the edge of the Adirondack forests, is today quite unknown, but eighty years ago it was an intellectual center of no slight importance. It was the seat of one of the best academies in New York State, and of a medical school which enjoyed a period of brief but rather brilliant prosperity. In this institution James Hadley's father was professor of chemistry and materia medica. A graduate of Dartmouth, he had a genuine enthusiasm for scientific study at a time when such study was rarer and less appreciated than it now is. It was through the influence of Professor James Hadley the elder that Asa Gray, while attending Fairfield Medical College, was turned from the practice of medicine to the pursuit of science as a calling. All of his sons who lived to maturity achieved a more than ordinary degree of success in study and teaching. The eldest, George Hadley, was for a long time professor of chemistry in Buffalo Medical College; the youngest, Henry Hadley, became professor of Hebrew in Union Theological Seminary, and was, by his untimely death in the service of the Sanitary Commission, cut off in the beginning of a career of great promise in the development of Semitic philology and Biblical criticism.

Amid such surroundings James Hadley's natural taste for study received every encouragement, and his scholarly tendencies were perhaps confirmed by an unfortunate accident to his knee-joint in boyish play, which left him lame for life and deprived him of the possibility of taking part in the sports of his fellows, which he would otherwise have so much enjoyed. After finishing the regular course at Fairfield Academy, he became an assistant in the teaching force of that institution. At the age of nineteen he entered Yale as a junior in the class of 1842, and two years later graduated with the very highest honors. He was appointed a tutor at Yale in 1845, an assistant professor of the Greek language and literature in 1848, and a full professor in 1851.

During the early period of his education he had shown no marked predilection for any one group of studies. He had a universal interest in books and a singularly catholic power of appreciating and handling every form of literature or science. His mathematical work both before and after graduation attracted great attention from Professor Peirce, who was much disappointed that Professor Hadley's final choice of lifework lay in the direction of philology rather than of mathematics. The special impulse in the direction of philological study came largely from the teaching of Mr. Edward Elbridge Salisbury. Mr. Salisbury was a scholar of the older stamp, appreciative rather than original, and content to instruct those who came in his way without seeking the wider reputation which published works might give; but he had studied under the best masters, and the Sanskrit class where he was the teacher and James Hadley and William Dwight Whitney the two pupils was not surpassed for efficiency in any of the universities of the Old World. The friendship formed in this class between Mr. Hadley and Mr. Whitney was one that lasted all through life, and furnished the very strongest means of personal enjoyment and scientific stimulus to them both. Whitney was able to continue his studies in Europe, both in the special line of Sanskrit and in the broader field of comparative philology. Hadley was kept at home by the necessities of teaching, and was led by force of circumstances to apply the results of his philological study chiefly to the problems presented by the Greek and Latin languages. This restriction was by no means an unhappy one. His teaching duties, though exacting, were congenial. He handled classes well and enjoyed doing it; and he enjoyed still more fully the opportunities of acquaintance with the individuals in each class who were able to appreciate his scholarship and be inspired by it. His marriage to Miss Anne Twining, of New Haven, was thoroughly happy, and in his home life he showed at its best the affectionateness of disposition, the calm serenity of temper, and the brilliancy of wit and power of conversation in which he stood so preëminent.

His health was habitually good, but never robust. The anxieties incident to the period of the civil war, culminating in the death of his brother Henry, with whom he had been most closely associated, led to a serious illness in the year 1865, which interrupted his work of undergraduate teaching. From the effects of this illness he never fully recovered. In October, 1872, he was seized with a malady which baffled the discernment of his physicians and proved fatal after a very brief time. He died on November 14, 1872.

James Hadley, like William Dwight Whitney, did his philological work in the period when the students of that science were under the dominant influence of Bopp. The value of the Sanskrit grammar as a basis of comparative philology had been newly discovered. It seemed to furnish a key to the whole development of Europe, and of many parts of Asia, in the time immediately preceding historic records. In the one-sided pursuance of this idea many mistakes were naturally made by the philologists of this generation. But they were mistakes of the right kind—mistakes incident to the vigorous use of a new discovery, where much new scientific truth is being developed. Into this movement of scientific discovery Professor Hadley entered heart and soul, and he took his full share in contributing to the etymological and phonetic discoveries of the period. Of his single pieces of original work, the one which was probably most widely known (having been at once translated into German, under the direction of George Curtius, at a time when the work of American scholars was practically unknown to their German brethren), related to the nature and theory of the Greek—and incidentally of the Latin—accent. His investigations showed how a system of accents which was apparently different in the two languages, and in one of them at least quite arbitrary, could by proper analysis be reduced to certain very simple rules of musical modulation. The contrast between this treatment of the subject of accent and that which had passed current among English-speaking scholars up to that day was so great that a reviewer, comparing the work of Professor Hadley with a contemporaneous essay of Professor Blackie, of Edinburgh, on the same subject, said that it did not seem as though the two things could have been the product of the same century.

Had Professor Hadley lived in Germany, or had the America of fifty years ago been like that of today, his scientific papers would have found larger audiences than could be furnished by the Classical and Philological Club of New Haven, and perhaps more distinctively suitable ones than those of the American

Oriental Society. But nowhere in the world could he have found college classes which would have given him a better field of influence on the thoughts of a new generation. This opportunity he was able to use to the utmost. He was the first of our classical teachers who really brought home to his pupils sound ideas of word structure and word formation. The "Analysis of the Greek Verb," which he placed in the hands of freshmen at Yale, was an application of the methods of modern science to explain some of the complex phenomena of the Greek language. One or two of the propositions laid down in that Analysis have been modified by subsequent investigation; but the method remains good, and the classes who were taught by that method were grounded in scientific word study as no American classes ever were grounded before. The same ways of looking at language were applied on a larger scale in his Greek grammar, founded on the German work of Curtius, and following that work in its general arrangement, but with so many differences as to make it really an independent book. If any one will compare Hadley's Greek Grammar, published in 1859, with any of the Greek or Latin grammars used by American students before it, and with most of those which appeared for a good many years afterward, he will see the radical character of the change from old scholastic formulas to modern scientific principles which the introduction of this book involved. It was indeed too modern in conception for many of the teachers who had been trained under the old rules and formulas; but as habits of scientific study advanced, its influence was more notably felt with each recurring year.

The closing years of Professor Hadley's life witnessed the development in America of courses of graduate instruction. In these developments he took the very greatest interest. He was active both in organization and in the work of teaching. Brief as was the time of his activity in this field, it was sufficient to give him a large influence on the next generation of philologists. In the list of his graduate students in the closing years of his life were included the names of Easton, Edgren, Lanman, Learned, Luquiens, Manatt, Owen, Perrin, Sherman, and Minton Warren. A series of names like this is perhaps the best monument of the scientific work which was being done under his direction.

But it would be a mistake to assume that Professor Hadley's influence as a teacher was that of a mere philologist. With his love of the study of language he combined an equal love of the study of literature. The work of teaching Greek gave him an opportunity to manifest his power in both of these directions, and to give to men of the artistic temperament as well as to those of the scientific temperament the treasures which the study of Greek had in store for them. His translations, whether extempore or deliberate, were full of charm and preserved in a wonderful degree the rhetorical spirit of the original as well as its grammatical meaning. His reading had been phenomenally wide. When Trevelyan's Life of Macaulay appeared, English students as well as American were filled with surprise at the extent of knowledge of classical authors which he had acquired at school and at the university; but James Hadley had read as much Greek and Latin before he left Fairfield as Macaulay had read during his whole school and university life. He had studied extensively and critically in ancient jurisprudence, and, following out a natural taste for legal theories and using the singular power of lucid statement which he possessed, he wrote a series of lectures on Roman law which, published just after his death, are today in some respects the best elementary English exposition of the system of Gaius and Justinian. Nor were his literary acquirements confined to classical fields alone. He was widely read in German and in French, in Scandinavian and even in Celtic—the Welsh literature being a special favorite with him. Nor should his interest in the study of English be forgotten; for at a time when American colleges and schools were teaching foreign languages to the neglect of our own, Professor Hadley wrote his brief history of the English language, which, with some revision by Professor Kittredge, still serves as an introduction to Webster's Dictionary; and it was in no small measure through his influence that Professor Lounsbury was induced to accept and develop the chair of English in the Sheffield Scientific School to which he has since brought so much honor.

Professor Hadley was from a very early period active in the affairs of the American Oriental Society; and somewhat later, when the time was ripe for the formation of the American Philological Association, he was prominent as one of its organizers and took constant and conspicuous part in its discussions. At the time of his death he was president of the former body and vice-president of the latter. His published papers, scattered through the transactions of the American Oriental Society and through journals whose files are less accessible, form a list too long for detailed citation. Some of the most important ones, selected with reverent care by his friend Professor Whitney, were collected under the title "Essays, Philological and Critical," and were published in New York in the year following his death. The titles of these various essays show the wide range of their author's interests, from an investigation of the original seat of the Ionians on the one hand to an examination of the prospects of republicanism in Europe on the other; from studies of Greek roots and Greek rhythms to critical appreciations of the poetry of Tennyson. Some of these essays were the product of long and careful study, others were mere accidents of his daily activity; but, whatever the circumstances of their origin, they all show the close observation, the clear deduction from premises, and the power of accurate statement which made his work as a teacher so preëminent.

BIOGRAPHICAL MEMOIR

OF

HENRY BARKER HILL,

1849-1903.

BY

CHARLES LORING JACKSON.

READ BEFORE THE NATIONAL ACADEMY OF SCIENCES,
APRIL 21, 1904.

BIOGRAPHICAL MEMOIR OF HENRY BARKER HILL.

Henry Barker Hill was born on the 27th of April, 1849, at Waltham, Massachusetts, where his father, the Rev. Thomas Hill, was the clergyman of the Unitarian Church. His paternal grandfather came to this country from Warwickshire, in England, and settled in New Brunswick, New Jersey. His mother, whose maiden name was Anne Foster Bellows, belonged to a family long conspicuous in the history of New Hampshire and Vermont.

Dr. Thomas Hill was not only eminent in the Unitarian ministry and a man of broad and varied attainments in science and literature, but was also one of the most profound yet brilliant mathematicians in the country. The active, inquiring quality of his mind led him into many fields of research, and showed itself even in his amusements, as he delighted in puzzles and similar mental gymnastics. Young Hill, growing up under this singularly inspiring influence, was marked out by inheritance and environment for an investigator.

At ten years of age he went with his family to Yellow Springs, Ohio, where Dr. Hill succeeded Horace Mann as president of Antioch College, and was plunged into the academic atmosphere, which he was to breathe for the rest of his life.

In 1862, at the age of thirteen, he entered the freshman class of Antioch College, but left it before the year was out to accompany his father to Cambridge, whither he was called as president of Harvard University. Here he attended the Cambridge High School, and later passed through Harvard College, from which he graduated in 1869. The elective system was then in its infancy, and Hill devoted the few courses in which choice was allowed principally to chemistry, mathematics, and music. He was such an eminent scholar in each of these departments that many of his classmates were astonished when they heard at graduation that he had decided to be a chemist, as they had connected him rather with one of the other subjects. As a matter of fact, however, he had not only taken both of the elective courses in chemistry, but had devoted much time in his senior year to an extra course on advanced crystallography.

The year after his graduation was spent in the Berlin laboratory under A. W. Hofmann in laying the foundations of a chemical education. Here he met his classmate, Mr. Edward L. Burlingame, who was living with his father, Anson Burlingame, then Chinese envoy at the Court of Prussia. The acquaintance between the two young men soon ripened into a close friendship, and this was of great importance to Hill, as it brought him into a cultivated diplomatic society presided over by Bancroft, the historian. Up to that time his retiring disposition had prevented him from associating with more than a few friends, and this wider range of associates had an excellent influence in the formation of his character.

When he went to Germany he had intended to stay for several years, in order to finish his chemical education, but at the end of the first year he was offered the position of Second Assistant in Chemistry in Harvard College. This offer was accompanied by a letter from his father advisiug him to accept the place, as he thought the chemical department of the college was on the eve of a marked growth. Hill cut short his scientific education and returned to Cambridge with many misgivings, but his subsequent career proved the wisdom of this step.

His chemical training at this time was slender. In Harvard College he had taken one course in qualitative analysis, one in mineralogy, and the advanced crystallography already mentioned. In Berlin he had devoted one semester to quantitative analysis, the second to organic chemistry, and had heard the lectures on inorganic and organic chemistry. He came home full of enthusiasm for original work, and determined to do his share toward the advancement of the science, but with neither teaching nor experience in research.

His duties at first consisted in teaching theoretical chemistry and qualitative analysis, to which, in 1874, he added a course in organic chemistry; and these two latter subjects, with the addition of quantitative analysis for a few years, made up the bulk of his teaching work during the rest of his life. In 1891 he served as lecturer on organic chemistry for a year at the Massachusetts Institute of Technology.

In his teaching he was not content to follow slavishly the ideas of others, or to drop into a routine; thus working out the principles established by Professor Cooke, he converted qualitative analysis from a purely mechanical affair into one of the best of educational disciplines. A small book embodying his ideas on this subject—Lecture Notes on Qualitative Analysis—is his only published volume.

His course in organic chemistry was a model of comprehensiveness, and of the preservation of the just relation between the important and the unimportant. It was kept abreast of the times—no light task in a science growing so rapidly—and in many instances was even in advance of them, as his penetrating judgment often led him to conclusions only accepted by the chemical world long afterward—one notable example of this is his anticipation by many years of the present theory of the constitution of the diazo compounds. His early mathematical training also showed itself in his calculation of the number of isomeres possible for various formulas—a subject treated with great fullness and vividness in his lectures.

In 1871 he married Miss Ellen Grace Shepard, of Dorchester, who survives him. Their son, Edward Burlingame Hill, inherited one of his father's marked tastes, and is a successful musician. Hill was an affectionate and devoted husband and father, and his chief happiness lay in his domestic life.

As his salary from the college was for a long time far below his moderate wants, he turned his attention during these early years to applied chemistry, and made a short investigation of the amount of carbonic dioxide in the air and an exhaustive study of the adulterations in confectionery sold in Massachusetts. Both of these were made for the State Board of Health and described in its reports. He also did some chemical work for a bleachery, and later was for many years the consulting chemist of Carter and Company, the manufacturers of ink. In this commercial work Hill for the first time showed his high quality as a chemist, for the overflowing enthusiasm for research with which he came home from Germany had met with many obstacles; his training, as has been said, was meager, and there was no organic chemist at Cambridge to help him finish his education, his laboratory accommodations at first were inadequate, consisting of a corner in the large public laboratory, until in 1875 he shared a small private laboratory with the present writer, and, worst of all, his time and energy were taken up by his duties as a teacher and by his commercial work. But he was not the man to be turned aside from his purpose even by such difficulties, and not a year passed in which he did not make progress, either by the necessary preliminary studies or by vigorous attacks on some field of research. At first these efforts led to no results, but with the patience and perseverance which were among his most marked characteristics he considered these defeats part of the training necessary for success, and at last, in 1876, he attained it in the publication of his first scientific paper, which was remarkable in showing none of the crudeness and uncertainty of touch usually characteristic of first papers, but was a well selected research, treated with the vigor, scientific insight, and beauty of finish of a mature chemist. This paper was on the methyl ethers of uric acid, and by following the fate of the methyl in the decomposition products he succeeded in throwing much light on the structure of this perplexing substance. This method of attack, originated by him, has lately, in the hands of Professor Emil Fischer, led to the determination of the constitution of uric acid.

The work for the ink factory already mentioned now had, indirectly, a commanding influence on his scientific career, as, in behalf of this firm, he visited the pharmaceutical factory of Dr. Squibb in Brooklyn to see some improved forms of percolators. During this visit Dr. Squibb showed him a new process for making acetic acid by the distillation of oak wood at temperatures from 150° to 200°, and asked him if he wished to examine the waste products. On his joyful acceptance of the offer a large amount of material was sent him, in which he discovered furfurol, at that time an almost unstudied substance, since its price, according to the catalogues, was $80 a kilogram.

While Hill was making preparations to use this happy chance to the utmost, a paper reached him in which Professor von Baeyer reserved the whole field for his own work; but when he heard of Hill's unrivaled opportunity he most generously resigned to him the investigation of mucobromic and mucochloric acids, and later, after the publication of a few papers, retired from the field, leaving it to Hill.

The work on uric acid, in spite of its interest and promise, was now dropped, after the publication of two papers, and the investigation of the derivatives of furfurol taken up, which occupied Hill's attention to the end of his life. His work in this field remains an enduring monument to his memory, for he converted the furfurane compounds from an unexplored waste into one of the best known of the larger domains of chemistry.

This work was described in over thirty papers, and among his most striking achievements may be noted the full study of mucobromic and mucochloric acids, with the determination of their puzzling constitution and the investigation of numerous substituted propionic, acrylic, and propiolic acids derived from them; the description of the brom- and chlorpyromucic acids, in which the interesting fact was brought out that these two halogens differ in their behavior with pyromucic acid; the study of the nitro and sulphonic derivatives of the furfurane compounds, with a careful comparison of the substances belonging to this group, with the corresponding aromatic bodies; the discovery of nitromalonic aldehyde and a long line of brilliant aromatic syntheses from it; a study of methylfurfurol, found in the fractions boiling above furfurol, and of its most important derivatives.

Many years later the manufacture of acetic acid by this process was abandoned, and furfurol once more became a rare substance; but as this misfortune seemed about to paralyze his work, he contrived an improvement in the manufacture of dehydromucic acid (a derivative of furfurane), which made this substance very accessible and gave him fresh material for the study of this group. This was no lucky chance like the discovery of furfurol, but the intentional improvement of an old process worked out with great sagacity.

At the time of his death he was investigating pyrazol compounds from the oxime of nitromalonic aldehyde, and the reduction products of dehydromucic acid, some of which he had succeeded in separating into the optically active forms.

An examination of these papers brings out Hill's great qualities as a chemist—his grasp of the subject and power of close and logical reasoning, his uncommon experimental ability, and above all the thoroughness and accuracy which were his most striking and peculiar characteristics. Most chemists are content to accept the work of their students after testing it in two or three places, but he was never willing to publish until he had repeated the whole of it with his own hands, and if this diminished the number of his papers, it gave them a finish and authority rarely found in those of others. His papers, too, are written in a clear, finished, and beautiful style, unfortunately not too common in purely scientific articles.

From his students in research he exacted the same thoroughness and accuracy he used in his own work, and while this overtaxed the patience of a few, most of them became filled with his spirit, and all felt toward him an enthusiastic admiration and affection.

In 1874 he was made assistant professor, in 1884 full professor, and in 1894, on the death of Professor J. P. Cooke, he was appointed Director of the Chemical Laboratory. He was then confronted with the problem of forcing the rapidly growing department into an antiquated building, originally planned for forty students, with walls, both inside and out, of hampering massiveness. Under these discouraging conditions he contrived adequate laboratories for seven hundred students, enriched with new forms of hoods, water baths, and other apparatus, which revolutionized the methods of chemical architecture.

He was equally efficient in the organization of the business affairs of the department, so that this large establishment was managed with a minimum of work, and yet with perfect accuracy in every detail. It was no vain praise when he was called by the best authority the ideal director. These extraordinary results were not obtained without grave sacrifices on his part. For three years he could give but little attention to his original work, and even after the department was reorganized he insisted with characteristic thoroughness on attending to a multitude of details, which took up time that could be ill spared from research.

The ingenuity and mechanical skill shown in contriving the hoods and water baths in the public rooms also appeared in many new devices for the furtherance of his researches and placed carpentry and cabinet-making among his favorite amusements.

He was elected into the National Academy of Sciences in 1883, and was also a member of the American Academy of Arts and Sciences, the New York Academy of Sciences, the Washington Academy, and the German and American Chemical Societies. He had little time and less taste for the meetings of learned societies, or the faculty of the college, and was particularly averse to taking part in public discussion from an entirely unwarranted distrust of his ability as a debater, but in committee work his administrative power and sound judgment were of great use to the university, especially in the practical reorganization of the Lawrence Scientific School. At the time of his death he was a member of the Standing Committee of the Parish of the First Church of Boston.

With all this devotion to his profession he was no narrow specialist; his reading was wide and judicious in more than one language; he was a genealogist and musician of no mean attainments, and he possessed a truly astounding wealth of information on the most varied subjects. His disposition was naturally retiring, and these various employments left him little leisure for society; in fact, in Cambridge his life was almost that of a recluse, but the narrowing tendency of such a life was entirely counteracted by the pleasant social intercourse of his summers, passed during the last part of his life in Dublin, New Hampshire, where he showed himself such a charming companion that the necessity of his retired life during term time was the more regretted.

He never spared himself in anything, even in his recreations, maintaining that he preferred to wear out rather than to rust out; his health, therefore, was far from good, the days when he was free from headache and dizziness being the exception rather than the rule, but he never gave up. Frequently he has lectured when most other men would have been in bed. These uncomfortable attacks seemed, however, rather unpleasant than dangerous, and even after more serious symptoms had appeared his friends hardly realized their import; so that it came as a surprise to most when, on April 1, 1903, he was seized with a violent malady, sank rapidly, and died on April 6.

He was known to the world by his work; but his friends delight to dwell on his patience, unselfishness, and modesty; his warm, affectionate nature; the fortitude which never allowed bodily weakness to interfere with his duties; the poise and sanity of his judgment, and, above all, on the entire sincerity, the aggressive honesty, which were the expression of the ruling principle of his life—an almost passionate devotion to truth.

A LIST OF THE PRINCIPAL CHEMICAL PAPERS BY HENRY BARKER HILL.

1. On the Ethers of Uric Acid. (First Paper.) Proc. Amer. Acad., **12**, 26. Sill. Amer. Journ. (3), **12**, 428.
2. On the Ethers of Uric Acid. (Second Paper.) Dimethyluric Acid. With C. F. Mabery. Proc. Amer. Acad., **15**, 256. Amer. Chem. Journ., **2**, 305.
3. Furfurol One of the Products of the Dry Distillation of Wood. Proc. Amer. Acad., **16**, 156. Amer. Chem. Journ., **3**, 33.
4. Pyroxanthin. Proc. Amer. Acad., **16**, 161. Amer. Chem. Journ., **3**, 332.
5. Mucobromic Acid. Proc. Amer. Acad., **16**, 168.
6. Mucochloric Acid. Proc. Amer. Acad., **16**, 204.
7. Substituted Acrylic Acids from Brompropiolic Acid. Proc. Amer. Acad., **16**, 211.
8. Theoretical Considerations. Proc. Amer. Acad., **16**, 218.
9. On Dibromacrylic Acid. Proc. Amer. Acad., **17**, 125. Amer. Chem. Journ., **4**, 169.
10. On the Relation between Dibromacrylic Acid and Tribrompropionic Acids. With C. W. Andrews. Proc. Amer. Acad., **17**, 133. Amer. Chem. Journ., **4**, 176.
11. On Certain Tetrasubstituted Propionic Acids. With C. F. Mabery. Proc. Amer. Acad., **17**, 140. Amer. Chem. Journ., **4**, 263.
12. On the Constitution of the Substituted Acrylic and Propionic Acids. Proc. Amer. Acad., **17**, 150. Amer. Chem. Journ., **4**, 273.
13. On Mucophenoxybromic Acid. With E. K. Stevens. Proc. Amer. Acad., **19**, 262. Amer. Chem. Journ., **6**, 187.
14. On Substituted Pyromucic Acids. (First Paper.) With C. R. Sanger. Proc. Amer. Acad., **21**, 135. Ann. Chem. (Liebig), 232, 42.
15. On Mucoxybromic and Mucoxychloric Acids. With Arthur W. Palmer. Proc. Amer. Acad., **22**, 315. Amer. Chem. Journ., **9**, 147.
16. On the Minimum Point of Ignition of Decayed Wood. Proc. Amer. Acad., **22**, 482.
17. On the Decomposition of Wood at High Temperatures. With A. M. Comey. Proc. Amer. Acad., **22**, 488.
18. On Substituted Pyromucic Acids. (Second Paper.) On Sulphopyromucic Acids. With A. W. Palmer. Proc. Amer., Acad., **23**, 188. Amer. Chem. Journ., **10**, 373, 409.

19. On Chlorpyromucic Acids. With L. L. Jackson. Proc. Amer. Acad., **24**, 320 Amer. Chem. Journ., **12**, 22, 112.
20. On Certain Derivatives of Furfuracrylic Acid. With H. B. Gibson and C. F. Kahnweiler. Proc. Amer. Acad., **24**, 364. Amer. Chem. Journ., **12**, 314.
21. On the So-called Dioxymaleic Acid. With W. S. Hendrixson. Proc. Amer. Acad., **24**, 376. Amer. Chem. Journ., **12**, 325.
22. On Chlorsulphopyromucic Acids. With W. S. Hendrixson. Proc. Amer. Acad., **25**, 283. Amer. Chem. Journ., **15**, 145.
23. On Certain Products of the Dry Distillation of Wood. Methylfurfurol and Methylpyromucic Acid. With W. L. Jennings. Proc. Amer. Acad., **27**, 186. Amer. Chem. Journ., **15**, 159.
24. On Certain Derivatives of Pyromucamide. With C. E. Saunders. Proc. Amer. Acad., **27**, 214. Amer. Chem. Journ., **15**, 130.
25. On Certain Substituted Crotonolactones and Mucobromic Acid. With R. W. Cornelison. Proc. Amer. Acad., **29**, 1. Amer. Chem. Journ., **16**, 188, 277.
26. On Mucophenoxychloric Acid. With H. E. Sawyer. Proc. Amer. Acad., **30**, 242.
27. On the Oximes of Mucophenoxychloric and Mucophenoxybromic Acids. With J. A. Widtsoe. Amer. Chem. Journ., **19**, 627.
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29. On Certain Derivatives of Brommaleic and Chlormaleic Acid-aldehydes. With E. T. Allen. Amer. Chem. Journ., **19**, 650.
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31. On the Conversion of Methylpyromucic Acid into Aldehydopyromucic and Dehydromucic Acids. With H. E. Sawyer. Amer. Chem. Journ., **20**, 169.
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33. On Nitromalonic Aldehyde. With J. Torrey, Jr. Amer. Chem. Journ., **22**, 89.
34. Notiz über 2, 6 Diphenyl-4-nitrophenol und 2-Phenyl-4-nitrophenol. Ber. Deutsch. Chem. Ges., **33**, 1241.
35. On Nitromalonic Aldehyde. (Second Paper.) On the Condensation of Nitromalonic Aldehyde with Certain Ketones and Ketone Acids. With C. A. Soch and G. Oenslager. Amer. Chem. Journ, **24**, 1.
36. Dehydromucic Acid. Amer. Chem. Journ., **25**, 439.
37. On Dehydromucic Acid and Certain of its Derivatives. With I. K. Phelps and W. J. Hale. Amer. Chem. Journ., **25**, 445.
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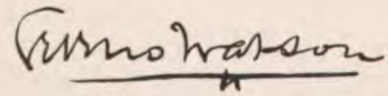
40. On the Oximes of Nitromalonic Aldehyde. With W. J. Hale. Amer. Chem. Journ., **29**, 253.
41. On Formiminoethylether. With O. F. Black.
42. On Nitrolactic Acid. With O. F. Black.
43. On the Optically Active Isomeres of β-Dihydrofurfurane-α-α-dicarboxylic Acid. With F. W. Russe.
44. On Tribrompyrazol. With O. F. Black.
45. A new Apparatus for the Determination of Melting Points.
46. Some Sulphamido Derivatives of Furfuran. With J. P. Sylvester.



BIOGRAPHICAL MEMOIR

OF

SERENO WATSON,

1820-1892.

BY

WILLIAM H. BREWER.

READ BEFORE THE NATIONAL ACADEMY OF SCIENCES,
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BIOGRAPHICAL MEMOIR OF SERENO WATSON.

Sereno Watson was the eighth son and ninth child in the family of thirteen children of Henry and Julia (Reed) Watson. He was born December 1, 1820, at East Windsor Hill, Connecticut, became a member of this Academy in April, 1889, and died in Cambridge, Massachusetts, March 9, 1892.

His parents were both of New England stock. His father descended from Robert Watson, who emigrated from the city of London and became one of the earliest settlers of Windsor, the oldest town in the state of Connecticut and the earliest permanent settlement in the valley of the Connecticut River. His descendants are still numerous in that region and are widely distributed elsewhere. The family has furnished a considerable number of persons of eminent ability in various vocations and activities in this country. His mother was a daughter of Dr. Elisha Reed, also of East Windsor Hill.

His father, Henry Watson, had been in earlier life a merchant, and on the death of his father, John Watson, retired to the ancestral farm near the village and there spent the latter years of his life. On this farm Sereno Watson spent his boyhood and developed that vigorous physique which was an important factor in his ultimately successful career.

This portion of the valley of the Connecticut is one of great beauty and fertility and is especially attractive. East Windsor Hill is the name given to a district with ill-defined geographical limits and without a separate municipal existence. It might be described as a country village and its surrounding farming neighborhood. It was originally a part of the old town of Windsor, but as population increased the town was divided and subdivided again and again. It was a part of East Windsor when Sereno was born, but for the last half century or more it has been a part of the town of South Windsor. The population is largely concentrated along a great thoroughfare—the old road which runs upon the second terrace which skirts the valley for many miles. East Windsor Hill is the swell of ground and table of terrace lying between the Scantic River on the north and Taylor brook on the south, two streams which cut into the terrace on the eastern side of the river, and some seven or eight miles northeasterly from Hartford. Here the wide street, adorned with several rows of trees, becomes a country village, with its pretty residences and gardens, and with post-office, shops, stores, etc., and, not the least important, East Windsor Hill Academy, where Sereno prepared for college.

In this neighborhood his ancestors had lived from the earliest settlement of this state, and, combining the quietude and industry of the farm with the business activities of a country village of those days, Henry Watson had lived on the main street in the village for some years, and removed into the country soon after the birth of Sereno. Amid these beautiful surroundings, on a farm endeared by family traditions, his youth was passed. The environment of his early life was peculiarly well suited to those tastes and faculties which ultimately turned his activities into scientific paths. No other industrial vocation is so favorable for the cultivation in youth of habits of observation, industry, and quiet study as is the life and work on an American farm. It was peculiarly adapted to his constitutional peculiarities.

He was by nature excessively diffident, retiring, reticent, and silent. He possessed keen powers of observation, a love of nature and nature study, combined with an excellent physique. He prepared for college at the East Windsor Hill Academy and entered Yale College in the spring of 1844, at the beginning of the third term of freshman year. His time there was quietly and studiously spent. He was so retiring that most of his classmates carried away with them only the college memories that he was of gentle and retiring disposition, and that he was a diligent student with a taste for the classics. The college records show that he took prizes in Latin composition and Latin translations, and he appears to have been what students now call "a good all-around man" in his studies. He did not room in the college buildings on the campus where the student life was mostly concentrated, but in private houses on quiet streets, and made comparatively few intimate college acquaintances.

He graduated in 1847, in his twenty-first year of age. This class has given to American botany two other earnest students, Professor Henry Griswold Jessup, of Dartmouth College, and

John Donnell Smith, of Baltimore, widely known for his investigations of the flora of Guatemala.

For many years after graduation his excessive diffidence stood sadly in the way of satisfactory success in any of the several vocations he attempted to pursue. These traits kept him in obscurity, and no one knew it better than he, as letters to his relatives and to some of his classmates abundantly show. Had he died twenty years after his graduation the world of science would never have heard of him, and most of his college mates would have considered his life a failure. He attended most of the meetings of his class during his lifetime. He greatly enjoyed these meetings, but there, as elsewhere, he was a man of few words.

He was continually conscious of his extreme diffidence and its repressing effect. In a letter to a relative in 1851 he says that "on leaving college I knew not what to do. I had no predilection for any of the professions," and that the only way that seemed open to him was teaching school. After graduation he remained at home three months ("on the anxious seat," as he later wrote a classmate), and then, from December, 1847, to April, 1848, he taught school at Scantic, in the town of East Windsor, Connecticut, and studied medicine with Dr. Watson of that place. From April to November of that year he taught school and continued the study of medicine at Flushing, Long Island. From November, 1848, to April, 1849, he taught at Warren, Rhode Island. It was the custom at that time for medical students to pursue their studies mostly in the private offices and under the direction of practicing physicians, and then supplement such instruction by lectures at some medical college. Accordingly, after leaving Warren, Rhode Island, he studied medicine with Dr. T. Sill, of Windsor, until November, 1849, then until March, 1850, attended lectures at the medical school of the University of New York, and then, as he told a friend, "left with a much diminished respect for medical practitioners and professors in general, apart from medicine itself, which is a noble profession." He resumed teaching in March, 1850, in Allentown, Pennsylvania, and in 1851 taught in Tarrytown, New York.

But his innate diffidence clung to him, teaching remained distasteful, and he went to farming again in his native place.

In 1852 his uncle, the Rev. Dr. Julius A. Reed, of Davenport, Iowa, one of the founders and a trustee of Iowa College, invited him to that institution, where he remained two years as tutor.

Before he had gone to Iowa, an elder brother, Dr. Louis Watson, a physician, in practice in Quincy, Illinois, had invited him to study in his office and enter practice in that town. He went there in July, 1854, studied with this brother, and entered on the practice of medicine and handled his cases well. But the practice was probably distasteful to him, for he gave it up in 1856, much to the regret of some of his patients. He then went to Greensboro, Alabama, as secretary of the Planters' Insurance Company, of which his eldest brother, Henry Watson, was president. He remained there until after the war broke out, in 1861, and then came north and was engaged with Dr. Henry Barnard, of Hartford, Connecticut, in literary work, chiefly on the *Journal of Education*. When Dr. Barnard went to Washington as Commissioner of Education this service ceased, and in January, 1866, at the age of forty, Mr. Watson entered the Sheffield Scientific School of Yale University and pursued the studies of chemistry and mineralogy. He worked very diligently in these studies for about a year, but botany formed no part of his instruction while there. He was so much older than the other students that he held little intercourse with them and made no acquaintances other than those of persons he necessarily met.

When and where he began the study of botany is unknown, but most probably it was in connection with his medical studies and soon after he left college. He collected and determined plants when living in Illinois and in Alabama, and it is very probable that during many years he pursued botany in an amateur way for the pleasure it gave him and as a congenial recreation.

He sailed for California by way of Panama, probably in March, 1867, and apparently without definite plans for the future, further than that he hoped to find there a more congenial occupation than he had found East.

While in the laboratory of Yale the previous year, Californian matters were much talked about. The writer had recently become a professor in the Scientific School, coming there from the State Geological Survey of California, on which he had been at work from 1860 as first assistant. Clarence King, who had also been an assistant on the same survey, spent part of that winter in New Haven, dividing his time between this place and Washington, where he was inducing Congress to institute a topographical and geological survey of a belt of land along the route of the Pacific railroad (then under construction) from California to the eastern base of the Rocky Mountains. Mr. Watson must have known something about this, but if it had any influence whatever in his going to California, it was very indirect and remote. He never spoke to the writer on the matter, and he never saw Mr. King until the next year, when he went to the camp of the surveying party in Nevada.

He had been faithful and diligent in every work he had tried to do, in a wide variety of vocations and several localities, and other activities were offered him. During the year before he left for California, his friend, Mr. Barnard, wished his assistance in the U. S. Department of Education at Washington, to which he had been called. He declined this as he did other offers—one of going into the drug business at Selma, another inducement to buy a sawmill and go into the lumber business at Mobile—and we know not how many other things.

The facts appear to be that all these were distasteful to him, and it is probable that when he went to California he had no definite plan or place in view, but he simply hoped that he might find there some work that was more congenial to his tastes and less hindered by his retiring nature. Several things indicate that he had in his mind vague possibilities of settling on a ranch. A letter written to a friend from San Francisco, April 28, 1867, shows that he had had the matter of going to California on his mind for some time; that he had "been ready to start on short notice, but one thing after another turned up which involved the possibility of my not coming at all, so I was kept in a state of uncertainty and not able to say whether I was coming here or not;" and that "one Friday I found myself clear of all questions of the kind and, to give no more time for any more to come, I determined to take the steamer which sailed on Monday." He hastily packed, hurried to New York the next day, and immediately sailed. He says at that writing that he had "no idea of going to the mines," and that he was as yet "unsettled and do not know where I will be nor at what business." He was at Sacramento a few days later. He spent two or three months in the Sacramento valley, and when at Woodville he heard that the expedition under Clarence King had started across the mountains. He resolved to join it.

From the terminus of the railroad he set out alone and on foot, crossed the Sierra Nevada, and found his way to the camp of the party, which was then on the Truckee River below the present town of Wadsworth. The trip had been a hard one and he was unused to mountains and deserts. He reached the camp weary, dust-covered, and so footsore with his hard tramp that he carried his heavy boots with his luggage over his shoulder. The camp men were struck with his appearance and condition as he inquired for Mr. King, and he in turn was taken aback by the very youthful appearance of the commander of the party. He brought a letter of introduction from Dr. Barnard, whom Mr. King had known in Hartford, and was so earnestly anxious to join the expedition that, if there was no scientific work for him, he offered to accept any position the camp offered. He was engaged to assist in topography, observe barometer, and "make himself generally useful" in such ways as he could. He entered on this new career as a "volunteer" with "wages nominal," his official rank and duties sufficiently vague to include a vast range of possibilities.

Mr. W. W. Bailey (now professor of botany in Brown University) was the botanist in charge, but he was already weakened by fever and could illy withstand the hardships and labor of collecting in a desert region. Watson began to assist him in the care of the collections and to collect plants in connection with the topographical work assigned him. He had already a general knowledge of botany and some specific knowledge of the flora of several regions east of the Mississippi River, but the plants of that dry and partly desert country were all new to him and intensely interested him.

After twenty years of struggle, with numerous discouragements in the several vocations he had attempted and the mental discomforts which his innate diffidence had caused him, he had at last found the work for which, of all others, nature had fitted him and which fate had reserved for him. It was especially congenial to his tastes and mental peculiarities, and, moreover, his great physical strength and powers of endurance were important factors in his success in the exploration of the country he was now in. His untiring diligence, his keen observation of plants, his uncomplaining endurance of the many discomforts and hardships of desert campaigning, soon gave evidence of his zeal in scientific work, and his patient, kind, and gentle personality soon endeared him to the whole camp.

Camp life and exploration work were entirely new to him, and his early experiences would have exhausted the patience and repressed the zeal of most men. The camp mule, which he had to learn to pack and ride, was not the least of the new discoveries awaiting him. The nature of the difficulties to which camp life introduced him contributed to the amusement of his new acquaintances, who were all well seasoned to such work, but the calm patience with which these difficulties were overcome inspired respect at first and admiration later.

Mr. King, commander of the expedition, has said of him: "He impressed me as a man of work, grimly and conscientiously in earnest. * * * He smiled only as a forced concession to humor. * * * Everything pertaining to his duty was sacred. * * * He soon learned to ride, and after the first anxieties regarding his duties had worn off, he began to enjoy the campaign life and the weird scenery of the region with the greatest enthusiasm. Mr. Bailey became more and more subject to camp illness and at last gave up and went home to the East. * * * I then installed Watson in charge of the botany. He was then as nearly perfectly happy as I have ever seen a human being. * * * When the hereditary New England grimness vanished from his face and he wore a free, careless air, * * * the general tone was calmly happy, and so I believe he remained till his connection with the Fortieth Parallel Survey ceased."

His actual botanical career began July 16 in the valley of the Truckee River, where he began collecting plants along with his other work. He soon proved himself to be an eminently useful man to the party, and at the end of his first month he was placed on a permanent footing in the service. He remained technically in the topographical section until the next year, and was botanist only by virtue of that vague duty he assumed, on first joining the party, to "make himself useful in such ways as he could." That wider duty was a natural outcome of his innate kindness. It had been learned much earlier, and was never forgotten to the end of his life.

His collecting that year was in the western part of Nevada, and he wintered with the party in Carson City. Meanwhile Mr. Bailey's health grew worse. He resigned and returned to the Eastern States in March, 1868, and Dr. Watson was appointed to succeed him as "Botanist in Charge." This was technically the beginning of his professional career as a botanist.

His collecting in 1868 began in the Carson valley in April. Early in May the party took the field again and worked eastward from the Washoe through the Trinity, West Humboldt, Havallah, and the several other mountain ranges to Ruby River, and from there the East Humboldt Mountains were explored, and thence eastward through the intervening ranges to Ogden, in the Salt Lake valley.

In the spring of 1869 his collecting began at Salt Lake, the party working eastward into the Wasatch, Uintah, and other ranges of the Rocky Mountains. At the close of his field work in autumn, he went to New Haven, Connecticut, and immediately began work on his report at the herbarium of Professor Daniel C. Eaton, of Yale University.

In a letter written soon after to a botanical friend, he says: "My work is at Professor Eaton's house, where all my plants are. I spend from two to twelve hours a day upon them and it is going to be an everlasting job to work them up. It is the best and the largest collection that has ever been brought in by any government party and promises to yield a fair proportion of new species." He continued the work here about a year, and late in 1870 went to Cambridge, Massachusetts, to the Gray Herbarium of Harvard University, where he finished his report in August, 1871, and it was immediately issued.

This report constitutes the fifth volume of the publications of the "United States Geological Survey of the Fortieth Parallel," under the charge of Clarence King. It is a quarto volume of 578 pages, 426 of which constitute a descriptive "Catalogue of the Known Plants of Nevada and Utah," with descriptions of such as do not occur east of the Mississippi River. It is therefore a systematic and descriptive botany of a broad belt of country from the great plains across the Rocky Mountains and the Great

Basin to the Sierra Nevada. It contains a map of the region, and many of the descriptions of new species are illustrated by full plates. The catalogue embraces 1,325 species and is preceded by a general description of the climatic and physiographic features of the region and a study of the distribution of these species in regard to their varied and interesting environment, along with notes and observations on the effects of these conditions on the growth and habits of the plants, which conditions are very unlike those of any region east of the Mississippi River.

The publication of this volume may be said to have constituted an epoch in the history of the botanical literature of this country in that it was the first descriptive list of species of the whole known flora of any region of western North America. There was an immense literature relating to it, but it was all fragmentary and was distractingly scattered as to publication. There was not even a published list of the mere names of all the plants known to occur in any state, territory, or even county west of the great plains. The present writer begun collecting in California in 1860, and in 1864 had begun the preparation of the "Botany of California," but this work went on intermittently and was not completed until nine years after this "Catalogue" was printed, and then only by Dr. Watson's aid. He had by that time become much more familiar with the details of the flora of the western United States than any other botanist.

From the time when he was appointed the official botanist of the expedition by Clarence King, in 1868, until his death, he devoted himself continuously and most industriously to botany, although for several years after ceasing his connection with the Survey of the Fortieth Parallel he had no official position as botanist. He remained, however, at the Gray Herbarium and continued botanical work, beginning immediately a most valuable series of "Contributions to American Botany," the first of which appeared in 1873, the last in 1891, but a few months before his death. There were eighteen of these "Contributions," the first appearing in the *American Naturalist*, most of the others in the Proceedings of the American Academy, at Boston, of which society he had become a member.

These "Contributions" extended over nearly twenty years, the work being carried on while attending to other duties. New collections were continually coming in for his examination. Some of the results were published in special "Reports," while the "Contributions" contained odds and ends of various others and many side questions that arose incident to this work. They also include the elaboration and reports of various special collections; hence they are very varied in their character; but every one bears evidence of his great capacity for patient and critical work. He possessed to an eminent degree a most extensive knowledge of minor details along with the faculty of seeing their relations to the broader generalizations involved. Thus it came that the series included revisions of several whole families of plants, so far as they were represented in the North American flora, and of more than a score of genera distributed through other families. Hundreds of new species were described, new light thrown on a multitude of others, and innumerable puzzles were cleared up. They constitute a storehouse of information pertaining to American systematic botany, and every succeeding worker in this field has been greatly aided and helped by it. Perhaps no other series of publications representing so much similar work has ever been published which has called forth so little criticism. All showed painstaking care, and hasty judgment has never been imputed to him.

His "Bibliographical Index to North American Botany," the first volume of which appeared in 1878, illustrates better, perhaps, than anything else he did, his faculty for critical work in taxonomy, along with his wonderful capacity for patient drudgery in its accomplishment. It also played a directly important part in his subsequent botanical career.

With the publication of his first work he stepped suddenly into the very front rank of systematic botanists. He was before totally unknown in the botanical world except to the very few botanists who had personally known him during the preparation of the volume. He had kept hidden behind his innate reticence, and when his official connection with the Survey of the Fortieth Parallel ceased he was in a sense again thrown upon the world. He loved botany for its own sake; but, however deep this love, the old proverb that "love don't make the pot boil" applies to botanists as it does to other men. Professor Gray, then in his sixty-second year, and overburdened with his constantly increasing labors and responsibilities, was very anxious to retain Dr. Watson at the Herbarium and Botanical

Garden of Harvard University, but the means then at the command of the Botanical Department did not warrant an "appointment." Professor Gray, however, asked him to remain as his private assistant while he cast about for the means of tiding over the period until the hoped-for time should come when he might be taken on the official staff of the university. The preparation of the Index enabled this to be brought about.

The writer was then at work in the preparation of the botany of California. His work and that of Dr. Watson had much in common. Geographically they covered adjoining regions having many physiographic and climatic features in common. Many of the species were the same. The literature relating to them was the same, except, that pertaining to California was much greater, more scattered, and more difficult of access. No list of the western species had ever been assembled or collated, but now it had to be done, so far as these adjacent regions were involved. Each of us had begun this in his own way as a necessary tool in the production of his local flora, but on different plans. The writer's plan for the Californian species was the easier and simpler, being merely an alphabetical index of the names and synonyms with reference to publication and habitat. Dr. Watson in his list had included notes on botanical characters and systematic classification. The writer discussed with Professor Gray the desirability of an alphabetical index of references to all the species west of the Mississippi River and the value of these two lists as a good beginning in such a work. The result of various conferences on the matter was that the writer corresponded with botanists on the subject and raised by subscription a sum of money large enough to warrant Dr. Watson's remaining at Cambridge for at least a year or two and beginning this compilation. Had this work been confined to the limits originally considered, it might have been finished; but even then almost any one but him would have shrunk from it.

The plants of western North America had long attracted attention. Menzies, Haenke, Née, Mociño, and others had collected plants along the coast as early as the eighteenth century, and in the next fifty years more than a score of other botanical collectors had visited some part of the region. Botanists accompanying expeditions from nearly all the civilized countries had botanized along the whole coast. Nuttall had crossed the continent, numerous collectors had penetrated far inland, commercial botanists and seed collectors had been numerous and busy. The collections had been distributed into many herbaria and the seeds to many botanical gardens; and the new species found were described here and there in many sorts of publications scattered over Europe and America. Probably no single library in the world had all this literature.

With the acquisition of California and the settlement of the Oregon question, the discovery of gold in California, and the consequent rush of miners and emigrants to the West, the plant collecting had been greatly stimulated. More than sixty government expeditions of our own country had been into this region before the preparation of the Botany of California was begun; many of them had botanists attached; numerous collections had been examined and the results published in separate reports. So it is not strange that, though most of the species had been already described, when a new collection was made no one knew whether all its species had been dèscribed or, if so, when and where published. Of course, a burdensome and conflicting synonymy had arisen. Many of the species had a wide range in latitude. Mexican species from the south met Alaskan species from the north and had been described from both countries along with species collected in other lands.

To collect the American portions into one reference list would be an immense work, and in whatever shape published the labor must be tedious, time-consuming, uninteresting clerical drudgery. Dr. Watson did not shrink from this. He knew well what a boon it would be to all future systematic botanists of the country. He would extend its scope and usefulness, and thus the Index grew on his hands. It contained an alphabetical list of western plant names, but that was only a necessary part of the greater Index of all the recorded species of North America, with bibliographic references and a chronological arrangement of the synonymy. Only Part I, Polypetalæ (1878), was published. Although never completed, this small volume is probably more used for reference by American systematic botanists than any other work he wrote.

Late in 1873 he began to aid the writer on the botany of California. The next year a few liberal citizens of San Francisco provided means for its publication, the printing was soon begun, and in 1875 the writer, being unable to carry on the work longer, turned over all the material to him. The first volume appeared in 1876, the second in 1880. This was the first complete systematic botany to be issued of any part of western America.

His official connection with Harvard University began June 29, 1874, when he was appointed "Curator of the Gray Herbarium," which office he held until his death. In 1881 he was appointed "Instructor in Phytology," which office he held two years. From 1872, as the assistant of Professor Gray, he had much to do with the care of the Botanical Garden, and part of the time was practically its manager; but this came by virtue of his helpful nature and his relations to Professor Gray rather than by office under the corporation.

With the assistance of Professor John M. Coulter, Professor Gray had begun a revision of his "Manual of the Botany of the Northern United States," which was brought to a sudden close by his death in January, 1888, and the transfer of his copyrights to Harvard University. The revision was then assigned to Dr. Watson, with Professor Coulter, and was published in 1889–1890.

Without further reference to the varied character of Dr. Watson's other work, it is sufficient to state that the list of his publications appended to this sketch of his life amounts to about a hundred titles; but this is only a feeble indication of their actual amount. A number of the "Contributions" and other titles consist of several sections, each of which would have been a valuable paper of itself if published separately, and this would possibly have been done by any one more anxious for posthumous fame. About twenty of his titles were published in the Proceedings of the American Academy at Boston, over forty were contributed to seven or eight different scientific periodicals, six appeared as government publications, and the remainder in various other ways.

His place in the ranks of American botanists is well defined. He stood in the first rank in his special field—the Phænogamic portion of the Systematic Botany of North America. His work on mosses was first class, so far as it went, but it was relatively very limited, and his ramblings in other botanical fields, though varied, were not extensive. It was the quantity and quality of his original work in his own field that gave him his position. He was as conscientious as he was diligent, and his botanical work was done with the same sense of moral and religious duty that he carried into all the concerns of his private life. He did not work for fame or reward. He loved to work, he believed it was man's duty to work, and whatever he did he felt should be done truthfully and well; hence the laborious care with which his studious observations were made and through which he arrived at his conclusions. His own intellectual instincts were supplemented by college studies along the old line of a definite curriculum, and his literary ability further trained by his earlier work as teacher and that with Dr. Barnard on the *Journal of Education*. He was by nature a man of few words; the style of his descriptive botany is terse, clear, and sharp, leaving but small chance of dispute as to its meaning. Imperfections due to youth had worn off in the various vocations he had pursued previous to his botanical career, which was begun with mature judgment. By nature not given to hasty conclusions, his acquired habits of patient, diligent study, his reticent ways—all these, as well as other factors, combined to give him great aptitude for botanical work, while the amount and quality of that work gave him a name and fame in the world of science. It is the kind of work that endures.

In the revolutionary disturbances in the field of botanical nomenclature pertaining to sequences and synonymy, that came on during his later years, he naturally took a conservative position. His mental constitution, his critical study of the American flora, both as to the plants themselves and the names that had been given them, the enormous amount of work he had done in handling names while preparing his Index of North American Botany, the maturity of his judgment—all tended toward conservatism in the matter. With him botany was the science of plants. Names were necessary for their classification as means to an end, and when a name had been long and reasonably well established, he questioned the wisdom of new and radical changes founded on the theory that hereafter language might be rendered stable by establishing new rules. Changes in classification may and doubtless will occur in the further evolution of botanical science as our knowledge of plants increases and the genetic history of their species is better known. Such evolution of the science will no doubt necessitate changes in many specific names he created or used. To such changes he would be the last to object. But they will in no way or degree undo his work or set it aside. That was chiefly a critical study of the plants themselves, and it will remain an enduring contribution to our stock of knowledge, and his name and fame as a master of systematic botany will endure.

The last twenty-one years of his life were spent at Cambridge. Its quiet was broken by only three trips of any considerable length. In 1880 he made a trip into the Northwest in connection with the forest work of the Tenth Census. In 1885 he went on a collecting tour to Guatemala. In 1886, in company with his colleague, Professor George L. Goodale, he traveled three months in Europe, a most restful and beneficial trip to him, as well as a very enjoyable one.

The personal peculiarities which so suppressed him in early life were not serious hindrances in his later and happier botanical career. His innate reticence remained to the end, and it often seemed repellent to strangers and others not in his intimate acquaintance. Owing to this, his warm personal friendships were relatively few compared to what they might have been had he courted acquaintance or even met strangers with the cordiality which really existed in his heart. As it was, all who knew him closely, not merely esteemed him, but held him in affection. Those who knew him best loved him most. Professors Gray and Goodale, the two university colleagues who were in the more direct official relations with him, perhaps enjoyed his closest friendship. The latter says of him:

"Those who were engaged in neighboring fields of botanical investigation knew him as a faithful friend of few words. He was observed to carry on his researches in silence, seldom alluding to any special task in hand until it drew near completion, and even then only briefly. He was always ready to interrupt his studies to assist others in theirs. He would enter with unconcealed pleasure into the plans of others, but without ever speaking of his own. Hence it happens that his intimate friends, when called upon to speak in his memory, think first of the reserve and silence in which he worked."

He was, both by instinct and training, a student and a lover of nature.. He loved study for its own sake, and he loved plants because they were plants, and studied them with the keenest interest as well as with acute powers of observation. He was also by nature a calm and patient man, and these characteristics had been intensified by his religious convictions, and trained during the discouraging struggles of his earlier years. This calm patience kept with him to the end; it was seen in every phase of his work. Nothing excited him, nothing ruffled him, nothing disturbed him. The most monotonous clerical drudgery was not shirked, but calmly and uncomplainingly pursued. In the study he would stop his work to help another, kindly and graciously; in the field even the government mule did not anger him, his calm, patient persistence always overcoming the difficulties. Through all his early discouragements it is not known that he was ever despondent.

He talked none about himself and but little about his work, either while doing it or later. Sometimes the publication of an important paper would be the first intimation that he was at work upon it to any but his immediate associates, and all this silence was without any intentional concealment. It was merely a result of his silent ways and unobtrusive nature.

He did not shun company, but he did shun taking any public part or in any way being placed personally conspicuous. He attended the meetings of his college class whenever he could, and enjoyed them as silently as was possible. He was a constant attendant at the meetings of the American Academy in Boston, but his botanical papers, which enrich the Proceedings, were read by title; some short and less technical ones were read by the secretary. He attended a few meetings of the National Academy of Sciences at Washington with much pleasure, but he took no part in discussion at any public meeting.

He was a member of many scientific societies at home and abroad and received the degree of Doctor of Philosophy from Iowa University.

He was a member of the Congregational Church and a faithful attendant. One of his classmates, a clergyman, says of him:

"His was one of those true and gentle natures that can always be trusted." * * * "In the family he was self-denying and very thoughtful of the interests of others, doing many a kind act, the recipient of which knowing nothing of the source from which it came." * * * "He was a man of decidedly religious character, though he could seldom be induced to take any public part in religious exercises. He was fond of his church, and for years instructed in a Bible class."[1] * * * "He believed that he must work while the day lasted and with no reference to reward except the knowledge that he had done what he could. I doubt whether he ever thought of posthumous fame."

He was never married. About the middle of December, 1891, he was attacked with the epidemic influenza then prevailing in Cambridge, and which became complicated with dilatation of the heart. For some weeks his recovery seemed possible, and he often talked with hopeful longing of soon returning to work; but he grew weaker and weaker, until he peacefully passed away on the morning of March 9, 1892, in his sixty-sixth year. He was buried in the Harvard College lot in Mt. Auburn Cemetery, at Cambridge.

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- Pentstemon rotundifolius, p. 472, fig. 73.
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Cordia Greggii, var. Palmeri, p. 233, fig. 106.
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R. E. Rogers

BIOGRAPHICAL MEMOIR

OF

ROBERT EMPIE ROGERS,

1813-1884.

BY

EDGAR FAHS SMITH.

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BIOGRAPHICAL MEMOIR OF ROBERT EMPIE ROGERS.

In 1798 there landed in Philadelphia, directly from the Emerald Isle, one Patrick Kerr Rogers. He had received, through private tutors, a liberal and broad training. The hope had been entertained by some of his family that he would take orders, which he, however, was unwilling to do because of innate and strong opinions, " not rigidly orthodox ; " hence he drifted into commercial pursuits. His appearance in this country was in large measure due to the fact that just prior to the outbreak of the Irish rebellion in May, 1798, he had spread abroad, through newspapers, declarations hostile to his government, and his friends, fearing dire consequences for him, made haste and prevailed upon him to emigrate to a more congenial clime. In 1799 he served as tutor in the college of the University of Pennsylvania, beginning at the same time the study of medicine under the preceptorship of Dr. Benjamin Smith Barton. His medical degree from the University of Pennsylvania was received in June, 1802, and in the city directory for that year it is stated that Rogers lived at 55 Lombard Street, where he had presumably established a home shortly after his marriage to Hannah Blythe, " an intelligent woman, a year older than himself, endowed with a cheerful and affectionate disposition," who had come from Londonderry in 1794.

For ten years this excellent couple continued their home in the City of Brotherly Love. The husband practiced his profession, but devoted much time and energy to public lectures on botany and scientific subjects in general, seeking to popularize them, and it is recorded that at last he gave himself entirely to chemistry, " upon which he delivered, it is supposed, the first complete series of popular lectures ever given in this city (Philadelphia) or in the country." At the close of the year 1812 the family moved to Baltimore, where the father worked zealously on, amid very discouraging circumstances, until 1819, when his ability and efforts were recognized by his selection as the successor of Dr. Robert Hare to the chair of natural philosophy and mathematics in the College of William and Mary. " Dr. Rogers was soon settled in the Brofferton house, on the college campus, with his wife and four sons. He made all the apparatus required to illustrate his lectures. In this making and mending he was habitually attended by his sons, who thus acquired unusual facility in the use of tools for working wood and metals. He also prepared and printed a syllabus of his course of instruction." His surroundings were most congenial and stimulating. The future looked bright and rich in results, but in the midst of this happiness there came all too soon the grim messenger, claiming for his own the mother, who in the summer of 1820 succumbed to a severe fever, leaving to the care of their father her four boys—James Blythe Rogers, William Barton Rogers, Henry Darwin Rogers, and Robert Empie Rogers—the eldest in his eighteenth year and the youngest in his seventh. So strongly had Rogers and his family entrenched themselves in the hearts of their associates at Williamsburg in the brief space of a year, that the boys became "almost foster children in the families of the professors." A brief interim, during which the ties of affection between father and sons became closer and more profound, and the family circle was again severed—the father, the beloved companion, surrounded by his little band of disconsolate sons, died a victim of malarial fever August 1, 1828, in his fifty-second year.

The gift of Patrick Kerr Rogers and his wife Hannah to the upbuilding and progress of science in America was indeed munificent:

James Blythe Rogers, chemical manufacturer, geologist, teacher (the successor in 1847 of Robert Hare as professor of chemistry in the University of Pennsylvania);

William Barton Rogers, geologist, a founder and first president of the Massachusetts Institute of Technology;

Henry Darwin Rogers, professor in Dickinson College, in the University of Pennsylvania, geologist of the first Geological Survey of Pennsylvania, professor in the University of Glasgow; and

Robert Empie Rogers, who was born on March 29, 1813, in Baltimore.

The latter, the subject of the present sketch, was directed by the father in his early educational efforts, and upon his death this charge was assumed by James and William. It was a labor of love, for "their tastes and pursuits were similar." The middle name Empie was assumed by Robert "as a lasting token of his grateful appreciation of parental care bestowed upon him at William and Mary College after the death of his mother * * * by the Rev. Dr. Adam P. Empie and his wife."

Growing up, then, as Robert did, under the most favorable influences and assisted in every way by devoted and loyal brothers, inheriting, too, an affectionate disposition, it is not in the least surprising to fall upon lines filled with the tenderness expressed in the following postscript of a letter to his brother William:

"What can be more grateful to an affectionate heart than to find in others a sympathy and reciprocation of the same warm feelings it proffers. How doubly blessed I consider myself when I feel that in my brothers I have found such beings."

After due preparation a life career had, of course, to be chosen. There was uncertainty, as in the case of almost every lad, although it appears that the general opinion among the brothers was that Robert should become a civil engineer, and, acting no doubt in accordance with this wish, he really placed himself with a surveying party; but how long he remained with it is uncertain, for the work proved uncongenial to him and was abandoned; whereupon he wrote to William:

"In a letter to Henry, some time since, I stated, as I have before done to you, that my favorite desire always has been, and I thought always would be, to follow, if possible, in your career, to become an instructor; and as preparatory to some higher station, I thought I should like to have charge of a school, either of my own or become teacher in some flourishing establishment of the kind. Such an occupation, I think, would be a useful schooling for myself, for I conceive that at no time could I learn so fast as when teaching, for then I should be making practical application of what I would be myself acquiring, and while occupied I would have also a portion of time altogether apart to myself to devote in my own way to my own improvement."

Thus he wrote at twenty, and continuing his study of botany, geology, and mineralogy he later began medical studies and became a pupil of Robert Hare, in whose laboratory he worked most diligently till the completion of his course. In March, 1836, he received his medical degree from the University of Pennsylvania. The title of his graduating thesis was "Experiments on the blood, together with some new facts in regard to animal and vegetable structure illustrative of many of the most important phenomena of organic life, among them respiration, animal heat, venous circulation, secretion, and nutrition." It appeared in the "American Journal of the Medical Sciences" (vol. 18, p. 277). It was very fully illustrated by wood cuts of apparatus devised and constructed by the author. Most attention was given the phenomena of respiration. In the course of his discussion he remarked: "Experiment leads one to the belief that all the alkalinity beyond that originally found when blood is fresh is in consequence of the formation of a portion of ammonia. In all instances where I have witnessed an increase of *alkaline* reaction, I have detected the most unequivocal indications of *ammonia*," and then he proceeded to establish conclusive evidence of its elaboration from the blood by decomposition. Again: "I am led to the conclusion that the presence of coloring matter in the blood is always accompanied by that of the saline substances. * * * I am almost induced to believe that there is not in the blood any independent proximate principle whose *exclusive* province is to give color, but that the presence of some salt is essential to this result. * * * The statements of Macaire and Marcet that the *color* of the *coloring* matter of arterial and venous blood differs, and that the former is not so dark as the latter, as it proceeds upon the assumption that they succeeded in insulating the coloring matter, is obviously liable to objection. * * * In some experiments upon venous blood and arterial blood, made with the view of determining the relative amount of saline matter present in the colored washings obtained from the clots of each, I found that the incinerated residuum procured from the arterial clot was always richer in this ingredient than the other." He then considered the gas evolved from blood introduced into a vacuum before it had coagulated. Some supposed it to be carbonic acid, others that it was nitrogen. "My own experiments," he observed, "conducted with all possible precaution, have always failed in detecting this gas (carbonic acid) in venous blood, either during coagulation or subsequently."

In that section of the thesis relating to the action of animal and vegetable tissues we read:

"My first object of attention has been to find whether or not there is a disparity in the rate of passage of gases through different structures. For this purpose four short tubes were chosen, equal in length and diameter. A portion of fresh cuticle recently separated from the *cutis vera* was tied across one end of the first. Over the second was fastened a portion of peritoneum, over the third was a piece of mucous membrane, and over the fourth a very thin section of liver. These tubes being thus prepared and arranged over a mercurial trough, an equal measure of carbonic acid was passed up into each; a glass vessel was inverted over each of the tubes and filled with oxygen six times in volume of the carbonic acid in each tube. The opposite sides of the organic structures were thus in contact with different gaseous atmospheres. A rise of the mercury in each of the tubes was soon perceived, and the rate of movement was seen to be distinctly different in each. At the end of thirty minutes the experiment was suspended, being deemed satisfactory, and the mercury in the several tubes stood nearly as represented in the figure. We here perceive that from the third tube, where the mucous membrane was used, was the largest escape of the contained carbonic acid; a less proportion passed through the cuticle, a less share still through the peritoneum, and the least of all through the section of liver. * * * These inquiries were repeated and extended to other membranes with similar results. * * * As a deduction of the preceding determination, it seemed highly probable that by the use of certain tissues we might effect a separation of a particular gas from a mixture of two or more, so that by varying the tissue we might eliminate any gas at will, performing a species of proximate analysis. To test the truth of such an inference, two tubes were taken, and being bent into a rectangular elbow, one extremity of each was closed by a plate of metal perforated by a small round hole, corresponding to the caliber of the tube. A membrane being placed between the plates, they were then tightly clamped together. Thus arranged, I introduced into the leg of one of the tubes a measure of carbonic acid, a measure of oxygen, and a measure of hydrogen; and four measures of nitrogen were made to enter the other. The tissue employed in the first instance was mucous membrane. In fifteen minutes the mercury stood elevated in the first tube and depressed in the second; and the experiment being stopped, the contents of the latter were examined. It was found that nearly the whole augmentation of volume in this tube was due to carbonic acid. Cellular tissue was now substituted in place of the mucous membrane, and after a longer time than in the previous case, when a similar change had arisen in the volume of gas in the two tubes, the contents of that tube which previously held the nitrogen were inspected, and were found to consist of some carbonic acid, a still greater proportion of oxygen, and all the nitrogen previously present. These experiments were extended to vegetable tissues * * * and always the general results were analogous to those above in showing a diversity of action according to the particular tissue and gases employed."

This story of the diffusion of gases through membranes naturally suggested an extension of these laws of action to liquids. This was done, and by a unique series of experiments, involving the use of most diverse apparatus, he obtained the most convincing evidence of "the existence of an agency controlling the transmission of certain fluids in preference to others." "Perceiving that in many instances the liquids performed their movements in opposition to gravity, I was curious to ascertain if they would be able to overcome a greater mechanical resistance." The result of his experiments led him to say that "the force of transmission in this case (potassium sulphate) could not have been less than four atmospheres, and we are entitled to conclude that it would have been still greater had the membrane withstood the pressure. * * * The laws of the transmission of fluids through organic structures are exhibited in results which are equivalent to a species of chemical decomposition." He next by use of membranes proceeded to separate gold, silver, and several other metals from their solutions, and was "led irresistibly to attribute an important office to the membrane itself."

The remainder of the investigation is marked by the same leaning toward experimentation, preferring rather that experimental results should guide in deducing his theoretical observations than mere uncertain speculation. This first product of Robert Rogers' venture into the field of investigation shows great manipulative skill and the true spirit of a researcher. Whether the hand of the master, Robert Hare, was in any manner concerned in the progress of the study is not indicated. It seems to be an entirely independent contribution, and well deserved the recognition it received from the faculty to which it was presented.

The attainment of the doctorate accomplished, it would be but natural to look among those engaged in the practice of medicine to learn how the young physician was progressing away from the academic atmosphere—from the guiding influence of his brothers. Such a search would have failed to discover the doctor where one would imagine him to be, for the practice of his profession was not to his taste. He gave himself wholly to chemistry; so that it was not a surprise to learn that from 1836 to 1842 he served as chemist to the first Geological Survey of Pennsylvania, his brother Henry being the head of that survey. The only independent chemical study conducted in these years by Dr. Rogers related to the analysis of limestones (Journal Franklin Institute, vol. xxv, p. 158), and more especially to the separation of calcium from magnesium. The method adopted was the addition of sulphuric acid followed by alcohol. Martin H. Boyé was associated with him in this work. In March of 1842 he was chosen professor of general and applied chemistry in the University of Virginia. At last he had attained his heart's wish. He could now teach, which he did with signal ability. It was during this period also that investigation in the field of pure chemistry occupied his attention. In 1846, in conjunction with his brother William, he communicated to Silliman's Journal, 1, 428, a "new process for obtaining pure chlorine gas," which consisted in heating potassium bichromate with hydrochloric acid. The reaction was found to be quantitative in its yield of chlorine. It is a method of the text-books of the present day. In the same year the brothers published "On a New Process for obtaining Formic Acid, and on the Preparation of Aldehyde and Acetic Acid by the Use of the Bichromate of Potassa" (Silliman's Journal, vol. 2, 1846, pp. 18–24). The method generally in use at that time for the preparation of formic acid consisted in distilling a mixture of manganese dioxide, dilute sulphuric acid, and starch or sugar. The yield of formic acid by this method was inconsiderable; it was also largely admixed with numerous other products. The substitute offered by the brothers was to introduce into a retort of one quart capacity 800 grains of potassium bichromate and 10 cubic inches of water. The mixture was gently heated. Three hundred grains of powdered white sugar were then introduced and about one cubic inch of sulphuric acid injected upon the mixture. This addition of acid was made from time to time with care. The yield of formic acid was most satisfactory, and the authors remark: "On comparing this process with that commonly employed we are convinced of its superiority, first, on account of the exemption of the product from SO_2, and in a great degree from other impurities; second, from the much larger amount of formic acid obtained by it from an equal weight of the oxidizing material, sulphuric acid and starch or sugar; and third, from the ease with which the action is controlled. * * * By the new process, we procure about nine times as much formic acid from the same weight of the three reacting materials, as by that hitherto in use."

In making aldehyde they applied a mixture of bichromate of potash and sulphuric acid upon alcohol. In this use of bichromate they had been anticipated by Kane. Their method of procedure, however, contained many details of value. For instance, they observed: "We found that when alcohol is added in small quantities at a time to a mixture of the bichromate and sulphuric acid the distilled product is almost pure acetic acid, but when sulphuric acid is slowly dropped into a mixture of the salt and alcohol the liquid which passes over contains little else than aldehyde."

The next subject to engage their attention was "On the Volatility of Potassa and Soda and their Carbonates" (Proc. Am. Asso. Adv. Science, 1848). Numerous experiments were made "proving that these materials have a much higher volatility than hitherto recognized by chemists. * * * Reference was made to the bearings of this determination upon chemical analysis, first, as furnishing the means of recognizing the presence of the alkalies and the alkaline earths in mineral substances, such as the feldspars, hornblendes, epidotes, &c.; and, secondly, as indicating the probable large loss of the alkalies of vegetable matters through the intense heat used in converting them into ashes. Allusion was made, also, to the almost entire absence of the alkalies in the ashes of anthracite and other coals as dependent upon the intense heat operating in their combustion, and experiments were adduced to show that the coal, prior to the combustion, contained alkaline matter in a marked quantity.

The volatility of magnesia as compared with lime was spoken of as useful in distinguishing between magnesian and calcareous minerals." They also presented a communication to the American Association (1848) "On the Decomposition of Rocks by Meteoric Water," in which "the fact was stated that only one or two observations have hitherto been made by chemists to test in a direct and conclusive manner the power of water at ordinary temperatures to decompose rocky substances. * * * Experiments were accordingly applied to all the principal crystalline minerals containing alkalies and alkaline earths. They were of two kinds with each specimen, the one with pure distilled water, the other with water charged with carbonic acid." The difference in behavior was shown to be capable of furnishing a useful auxiliary means of extemporaneous qualitative analysis. "Experiments were also cited disproving the opinion, which appears to be received among chemists, that the feldspars, hornblendes, &c., are entirely unacted upon by sulphuric or hydrochloric acids." Evidence was adduced (Proc. Am. Asso., p. 95, 1848) that magnesium carbonate is more soluble than calcium carbonate in water impregnated with carbon dioxide.

In Silliman's Journal, volume 5, page 352, 1848, under the title "New Method of Determining the Carbon in Native and Artificial Graphites," the brothers allude to the difficulty usually encountered in oxidizing native graphite and the graphite in cast iron, and then proceed to say that their new method gave uniform and accurate results; hence they regarded it as worthy of the attention of analytical chemists. They say: "As a means of comparing the purity of the different varieties of native graphite we have found it entirely satisfactory. As applied to the still more important purpose of determining the entire amount of carbon in the several kinds of iron and steel, our experiments have not yet been sufficiently numerous to enable us to announce confidently upon its accuracy, but we are not without hopes of making it available also for this object." The oxidizing mixture in the method consisted of potassium bichromate and sulphuric acid. The apparatus used by the brothers was singularly like that used today in the wet determination of carbon. The communication is instructive and suggestive. It evinces thought and nicety in analysis.

At the time when these studies were being conducted it was customary in analyzing the diamond to burn the "gem either in the open air, in oxygen gas, or in some substances rich in oxygen, as nitrate of potassa." High temperatures were also required. Their experience with graphite induced the brothers to extend the same treatment to this more precious form of carbon, with the result, in their language, "that the diamond may be converted into carbonic acid in the liquid way and at a moderate heat by the reaction of a mixture of bichromate of potassa and sulphuric acid—in other words, by the oxidating power of chromic acid" (Am. Jour. Sci. and Arts, vol. 6, p. 110, 1848). The apparatus was almost identical with that used in the analysis of graphite, but precautions were necessary "to correct a slight error arising from the evolution of a minute amount of carbonic acid from the bichromate and sulphuric acid caused by the presence of a trace of organic matter or of carbonate in the former."

Another topic of extreme interest to our investigators was the absorption of carbonic acid by various liquids. The results were embodied in communications to the American Journal of Science and Arts, volume 5, page 114; volume 6, page 96, and Proceedings of the American Association for the Advancement of Science, 1850, page 298. The conclusions were, briefly, these: Thirty-three substances, including acids and salts, were tested as to their absorptive power. Among the results of interest may be noted "that sulphuric acid of the ordinary density absorbs at 60 degrees, under common pressure, about 94 per cent of its volume of carbon dioxide and Nordhausen sulphuric acid 125, the absorption by pure water at the same temperature and pressure being 98." The authors comment on what takes place by virtue of the absorption of this large volume of gas to what occurs in the drying apartment of the apparatus of Will and Fresenius. They declare that in using this instrument an error of from 3 to 4 per cent might readily arise. They enter like objections to processes of determining carbon dioxide as used by Boussingault and Lewy, Le Blanc, Orfila, and others. The absorption of carbonic acid by water was made for temperatures ranging from 32 degrees to 100 degrees "and a curve constructed representing the law of absorption as dependent on temperature." This was believed by the investigators to be a pioneer study upon the influence of temperature on gas absorption.

A problem confronting analysts of their day in examining mineral waters was the determination of the sulphur content present in many of the waters in the form of hydrogen sulphide, and a sulphide either of an alkali metal or of magnesium or calcium. The brothers had experienced difficulties in this problem. They gave it thought and then said:

"Suppose the mineral water to contain free hydrosulphuric acid, together with sulphides, say of potassium and magnesium, we may proceed as follows:"

1. Determine the total sulphur for a given volume of water by precipitation with cupric chloride or silver nitrate.

2. Conduct hydrogen gas (carefully purified) through an equal volume of water, and then pass the mixed gases into a silver nitrate or arsenious solution. The precipitate will contain the sulphur of the free hydrosulphuric acid in the mineral water.

3. Heat the flask containing the water through which hydrogen is passed, so that its contents boil gently. Supply the upper space of the flask with a strong stream of hydrogen. Under the boiling point the hydrogen which issues will contain no hydrogen sulphide, but when the liquid boils, the stream of vapor and hydrogen will show the presence of this substance, evolved by the decomposition of the sulphide of magnesium or calcium.

4. The only sulphur compound left in the water is the alkaline sulphide. Therefore add to its solution copper chloride or an arsenious compound. "The sum of this and the sulphur of the free hydrosulphuric acid subtracted from the total quantity of sulphur will give that of the sulphide of magnesium."

Carbonic acid was found to expel hydrogen sulphide much more rapidly than hydrogen. "Twenty-five cubic inches of Blue Lick (Ky.) water contained in a narrow-necked bottle were subjected to the washing action of a brisk stream of carbonic acid gas, previously purified by transmission through water. * * * In twenty minutes not a vestige of hydrosulphuric acid could be detected." Our authors therefore recommended this course when the mineral water was known to contain hydrogen sulphide only in the free state, for carbonic acid was capable of decomposing the sulphide present in such a water.

The details of the many experiments were incorporated in a communication to Silliman's Journal, volume 18, page 213, under the title "On the Use of Hydrogen Gas and Carbonic Acid Gas, to displace the Sulphuretted Hydrogen in the Analysis of Mineral Waters, &c."

At the session of the American Association for the Advancement of Science held in 1848, Dr. Rogers with his brother James contributed a paper entitled "On the alleged Insolubility of Copper in Hydrochloric Acid, with an Examination of Fuchs' Method for analyzing Iron Ores, Metallic Iron, &c.," in which it was demonstrated, contrary to the general view, that copper would dissolve in the acid in the absence of oxygen. The solution took place in the presence of hydrogen and also in carbonic acid. The metal dissolved in marked quantity to cupric chloride. There is no evidence of the freedom of the gases employed from air. It was assumed.

It was in conjunction with James that Dr. Robert Rogers compiled from the works of Turner and Gregory a volume designed to be a text-book on chemistry. It appeared in 1846. It included both inorganic and organic chemistry.

The time given to the laboratory, to his literary work, and to teaching made the period of Dr. Rogers' service in the University of Virginia truly most active. It must, too, have been most congenial, working as he did, now with one and then with another of his brothers, upon problems alike interesting to all. In studying the publications to which reference has been made in the preceding paragraphs one can not well determine the special work of each brother. Their problems were very probably the subject of frequent discussion among themselves, and the suggestions of one became the property of all. As another writer has so beautifully remarked, "Each followed his routine course, but often they engaged jointly in one investigation, so that the public sometimes confounded their labors and gave credit to one which truly belonged to another. Their works were frequently mentioned at home and abroad as of 'the brothers Rogers,' and always in respectful and kindly terms. Mistakes of the sort never disturbed the perfect harmony that always existed between them, as they might have done had the brothers been rivals or competitors for reputation."

This most interesting group of brothers, working thus in the spirit of the father, with the affection for one another inherited from the mother, received its first shock in the way of dissolution in 1852, when James, then professor of chemistry in the University of Pennsylvania, closed his eyes upon the scenes of this world. But his work was to be transmitted to a brother, for in August of that same year Robert was chosen to fill his place, and a few years later (1856) became the dean of the medical faculty. The entrance into this broader field brought to Dr. Rogers a greater variety of opportunities for the display of his mental powers. The most diverse problems presented themselves for solution.

In 1855 he gave to his students and the public his American edition of Lehmann's monumental work on Physiological Chemistry. In the years immediately following he was engaged in expert work of various kinds, in executing the trusts of the various societies with which he was a member, and in caring for the many duties which devolved upon him as an assistant surgeon in the Military Hospital (1863), where he sustained the loss of his right hand while showing to a woman the dangers which beset her in feeding a steam mangle. He speedily learned, however, "to write with his left hand and to use the right arm, beneath the shoulder, in prehension with notable skill in his experiments while lecturing." A deeper sorrow came to him about this time through the death of his wife, Fanny Montgomery, who had been for twenty years his true helpmate.

His venture into petroleum speculations in 1864 cost him great pecuniary losses. In 1872 he was called to render public service, being appointed, together with Dr. H. R. Linderman, to investigate the waste of silver in the mint of Philadelphia. It became necessary in this work to improve old and devise new methods of refining precious metals. Many of these were later adopted with profit. In the report of the Director of the Mint for 1875 that official in writing of the mint of San Francisco proceeds:

"The arranging of the plan of the refinery and its equipment was intrusted to Robert E. Rogers, professor of chemistry in the University of Pennsylvania, whose eminent qualifications as a chemist and metallurgist rendered him peculiarly qualified for this service, and who performed the duty assigned him in an entirely satisfactory manner."

His report upon the consolidated Virginia and California mine in Nevada entailed much perplexing study until he was enabled to give a fair estimate of its value.

While these and other claims occupied much of his time he was more or less active in opposing certain proposed movements of the trustees of the university, begun not long after the removal of that institution to the west of the Schuylkill River. The old medical curriculum covered two years. Now it was proposed to lengthen the course. It was not done at once. Much discussion prevailed. The trustees also maintained that the division of students' fees should be abolished, and that the professors should be the recipients of stated salaries, regardless of the numbers in attendance. There was more or less discontent, and Dr. Rogers, after serving for a period of a quarter of a century, quietly accepted in 1877 an election to a similar chair in Jefferson Medical College, where he was most cordially welcomed by a great class of medical students. This position he held until 1884, when he became emeritus professor, but died shortly after, in the same year, September 6, aged nearly 72 years. His second wife, Delia Saunders, whom he married in 1866, had passed away in 1883.

In 1837 Dr. Rogers became a member of the Academy of Natural Sciences and was most active in its affairs. He participated in the organization of the Association of American Geologists and Naturalists in 1840, which in 1847 changed its name to the American Association for the Advancement of Science. He was also a member of the American Medical Association (1852); the American Philosophical Society (1855), being in regular attendance at its sessions and serving in the council; of the College of Physicians (1857); chemist to the gas trust of Philadelphia from 1872–1884; member of the annual Assay Commission (United States) from 1874–1879, both years included; member of the Franklin Institute, and its president from 1875–1879. He was active on the committee which reported (1878) to the institute on the efficiency of dynamo machines and the dangers of electric lighting (1881).

In addition to his scientific investigations, his literary contributions, membership in various scientific organizations, all indicating a man of ability, Dr. Rogers was also "author of many inventions, notable among them the Rogers and Black steam boiler, and of several modifications and improvements of electric apparatus."

To some of these the following letter, addressed to Dr. Persifor Frazer, no doubt refers:

UNIVERSITY OF PENNSYLVANIA,
MEDICAL DEPARTMENT,
PHILADELPHIA, *August 10, 1872.*

MY DEAR SIR: Owing to my absence from the city at intervals and your letter of ——— having been forwarded after me, it has been delayed in coming to hand.

In reply to your inquiry in reference to having constructed a battery upon the plan I described at the American Philosophical Society, I would say that under any other circumstances I should not only not object, but, on the contrary, be much gratified to see you in possession of one in good working condition. If, however, it will not put you to inconvenience to delay a little in ordering it, I would prefer that you would do so that I may have an opportunity of publishing its construction in one of the journals.

You are aware that the fire in Jayne's building on Dock Street destroyed much of the material going through the press for the Philosophical Society. My own recent contributions were among those consumed, and unfortunately, not suspecting such an accident, I failed to preserve any copy of the one in question. I shall therefore be compelled to make fresh drawings and rewrite the article, which I shall do so soon as I can get through with the press of work now upon me.

Were an instrument-maker to furnish you the battery in the present posture of things, he might perhaps consider it so much his own construction, and possibly give to it publicity, which, among scientific men, would not be regarded as doing justice to the originator.

Feeling quite assured that you appreciate my motive in suggesting a delay in ordering the apparatus, I am,

Yours very truly,

R. E. ROGERS.

In 1883 Dickinson College (Pennsylvania) bestowed upon him its honorary degree of Doctor of Laws.

He was one of the original members of the National Academy of Sciences.

Dr. Rogers was popular among men. He was considerate of others. He had an intense interest in the welfare of his fellow-beings, and throughout his life always "had several decent poor people, old or enfeebled, depending upon his bounty, whom he cheered by familiar counsels and substantial gifts—little stipends to eke out their meager earnings." He was a man of courage, ever ready to serve in any emergency, and it is no little matter to know that three times in his life he rescued, at imminent peril to himself, from certain death persons wholly unknown to him. Two of these were snatched from a watery grave at Long Branch.

As a teacher he was beloved by his students. His lecture-room was always crowded. His gift of diction and his dexterity in experiment were very superior attractions, and, what is more, he constantly manifested a deep, sincere, personal interest in the every-day life and conduct of those whom he taught.

What another has said of the brothers Rogers may well conclude this sketch :

"Few have wrought more acceptably or more usefully."

"Who kindly shows a wanderer his way,
Lights, as it were, his torch from his own torch—
In kindling others' light, no less he shines."

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